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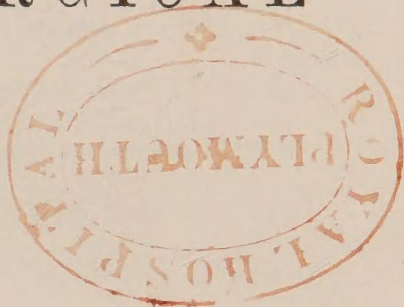
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THE
BRITISH AND FOREIGN
MEDICO-CHIRURGICAL
REVIEW



OR
QUARTERLY JOURNAL
OF
PRACTICAL MEDICINE AND SURGERY.

VOL. XLV.
JANUARY—APRIL, 1870.

LONDON:
JOHN CHURCHILL AND SONS, NEW BURLINGTON STREET.

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THE
BRITISH AND FOREIGN
MEDICO-CHIRURGICAL REVIEW.

JANUARY, 1870.

Analytical and Critical Reviews.

I.—Medical Politics.¹

ASSOCIATIONS formed for the purpose of exercising influence rather than of gaining legal powers are a new feature in our profession. Being a novelty, they are somewhat uncertain of their position, cautious in proclaiming their real character, and prone to disguise it by the assumption of scientific or benevolent intent. As we see no shame in the true bond of union, we shall, for the nonce, ignore these by-ends, and, without apology for lifting their veils, salute the British Medical Association and its twenty provincial branches, the Medico-Political Association, the Medical Teachers' Association, and several ethical societies scattered throughout the kingdom, as true political leagues. We welcome them with peculiar joy. Their use to the student of human affairs is very great, for they give him an opportunity of knowing the opinions of visible numbers of visible men, instead of trusting to their self-elected reflection in anonymous periodicals. It is needless to say that their primary object is to raise the rank of the profession in the commonwealth, and they come before the public as looking to the aid and protection of the State in various forms with this intent.

The introductory question which naturally must occur is—

¹ *The Medical Profession and its Educational and Licensing Bodies.* By E. D. Mapother, M.D., Professor of Anatomy and Physiology, Surgeon to St. Vincent's Hospital, &c. Dublin, 1868 (Carmichael Prize Essay).

General Medical Council; Report of the Committee on Professional Education, with Appendices. London, 1869.

Reports of Meeting of British Medical Association at Leeds, British Medical Journal, August and September, 1869.

Address of Samuel D. Gross, M.D., President of the American Medical Association. Philadelphia, 1868.

Why should the State interfere at all? What are the *gravamina* which its power is evoked to remove?

(1) In the first place medical men have to complain that the profession is rendered less influential as a body, and its individual members lessened in social consideration, by the legalised admission into their flock of incompetent practitioners, who do not receive, because they do not deserve, the confidence of their countrymen. Let any of our readers go to a chemist's shop in the artisans' quarter of any large town after work hours, and the crowds paying cash for advice and medicine, which they might at will get either gratis or on credit, or for a very small sum, from a registered practitioner, will make him hang his head and sadly confess that the millions do not receive the present legal guarantee as any evidence of the capacity of a medical man. Dr. Heslop, of Birmingham, in a terribly suggestive pamphlet on "The realities of medical attendance on the sick children of the poor in large towns," gives the statistical details of an inquiry which he has conducted as to the means adopted by a consecutive series of 384 parents among the lower classes for relieving the ailments of their offspring. He found that nearly half had been without any advice at all, fifteen, or two fifths of the whole, had obtained it solely from a druggist or herbalist, and only forty-two had been to a medical man. At a colliery, in which the present writer was interested, a neighbouring practitioner of some standing, M.D., M.R.C.S., L.R.C.P., London, L.S.A., was salaried to attend the underground hands. He enjoyed a sinecure; for a deputation from the gangs waited on the foreman and represented that the owner's kindness was thrown away; since they preferred, when ill, to take their burnt skins or disordered stomachs to what they suggestively called "the *regular* bonesetter." The unregistered practitioner was evidently viewed in the West Riding as a more orthodox institution than his titled rival. Examples of this temper among the working masses will occur to every one, if he will temporarily remove from his eyes that bandage which *esprit de corps* has lovingly bound round them. In the case which is the subject of the last anecdote the contempt for the authorized adviser was quite unmerited; but when we read in Dr. Heslop's pages "ignorance, recklessness, and hardness of heart," attributed to the established attendants on the labouring classes, we can easily understand the ill savour of the ointment, even though we should allow that no greater source of feter than dead flies appear on the surface. The distrust generated by individual instances is reflected upon our whole body. Ask an ordinary club cynic, who is declaiming against "the doctors," a reason for the faith, or rather want of faith, which is in him, and he is

sure to answer by citing cases of wholesale or retail stupidity ; and these are held to justify a disbelief, not only in physicians, but in physic altogether.

Now, the disgrace brought on us by unworthy members is, with a considerable amount of justice, ascribed to the imperfection of the tests which the State sanctions as enough to prove a man's competency to practise. Those who have the power of helping themselves reject those tests altogether. The army authorities submit candidates to a second examination, not on military but on general subjects. They consider that a medical adviser, who has been pronounced good enough to prescribe for the squire and the ploughman, is not proved thereby sufficiently expert to attend their brothers, the captain and the private. The Admiralty is equally careful of the lives of those who enter its service. Hospital authorities act in a similar way ; they require private certificates of professional ability from the candidates of house-surgeoncies, and sometimes they submit them to a special examination. County magistrates and conscientious guardians think nothing of the diplomas of those seeking their patronage, but, to justify their choice, demand testimonials by the score, which they are sadly apt to count instead of weighing.

(2) Another complaint is that the just gains of qualified persons are intercepted by those who, without any medical training, pass themselves off on the public as competent advisers. As they have spent no time or money on their education they require smaller profits, and, as they have not the knowledge needful for inquiring into the cases of the sick, they can dispense over the counter remedies much quicker and cheaper than any one who tries to cure the patient. The death-rate of the United Kingdom is wonderfully inflated through this broad-casting of drugs by chemists and other shop-keepers out of sheer ignorance, and without any bad intent.

(3) A still more serious evil to the State, though one can scarcely affirm it affects the profession, is license given to persons calling themselves "Coffinites," "Herbalists," or "Botanists;" who too often conceal the profitable trades of procurers and aborters beneath the thin veil of selling peculiar drugs. Under the same category come indecent advertisers and prurient museum-keepers. As Englishmen we may abhor their duplicity, but we cannot accuse them of injuring us as medical men. And we must also allow that Government gives them no direct encouragement, and tries to punish their offences after committal.

(4) We must, however, bring the very serious charge against Parliament of deriving a dirty addition to the revenue from

pledging its authority to patent quack medicines, by which the credit of the medical profession, and the viscera of the Queen's lieges, are injured in a much greater degree than the budget is benefited. People are encouraged, by the dignity of the Royal stamp and special protection afforded to these compounds, to think that in the view of our legislators the drug contributes everything, its administrator's knowledge nothing, towards the cure of disease. This partnership of our honoured Sovereign with a low class of retail dealers is a mode of money-getting compared with which the source of Vespasian's famous tax was cleanly—to the moral sense decidedly “OLET.”

(5) Medical men also complain that the mode of election, and the tenure of official appointments, under the Poor-law, are not such as to encourage those who seek them to acquire the higher qualities of head or heart. The guardians in populous places usually are of a class not fitted to judge of professional skill or natural tact; and the power to make the appointment of parish surgeon an annual one is a distinct notice that the stingiest ratepayer must have his avaricious cravings satisfied. The plan also of making the salary include the supply of medicines is a temptation to dishonest parsimony, which ought not to be placed before one who works hard for his bread. They complain also that struggling practitioners are driven to work on unremunerative salaries by the implied threat of introducing a rival which is held over them.

We might easily add to this list of grievances a good many which, perhaps even more than the above named, seriously afflict the souls of men ambitious and jealous of the honour of their chosen profession. But we are anxious to avoid all capable of being represented as sentimental complaints, and to confine ourselves to the most substantial and universally recognized evils.

Let us now come to the remedies proposed, on which we think it more convenient to comment as we go on, rather than to enumerate them first, and then to club our observations together at the end.

There appears to be a general consent, not only here but also in the United States, that the presence in our ranks of members morally and intellectually unworthy of the confidence of their countrymen is promoted and kept up by the multiplicity of Licensing Bodies employed by the State. It is said to be the interest of each of them to attract candidates by over-facility, and at the same time to hoodwink the outside world by a false show of high-class requirements. Doubtless the attraction of attention to the matter, and the bringing public opinion to bear by the formation of the Medical Council of Education, has of

late years arrested some of the most glaring abuses ; but in the statements made to the President of the Privy Council by Dr. Sibson,¹ himself lately an examiner for two of the Licensing Bodies, we have proof that the examinations are still very unsatisfactory. This gentleman spoke for a deputation from a large section of the British Medical Association, who represented "that the most important defect, carrying with it the greatest amount of evil, is the fact that in several of the examining bodies there is an absence of thoroughly practical, and especially of clinical, examinations." Now it is notorious that if one ventures on such a statement at a medical party, up jumps a graduate of Oxford, or of Cambridge, or of the London University, or of the metropolitan College of Physicians, and tells how their candidates are taken to the bedside and searchingly tested. But the advocate only speaks for one corporation out of nineteen, and by his defence furnishes strong proof of the justice of the accusation, for these very named boards are those which are the least patronized by candidates. Teachers can always find out which of their pupils are going to submit themselves to the efficient tests by observing that they frequent the hospital wards, and gain practical experience by personal intercourse with the sick, while the majority are rarely seen at this true fountain head of knowledge, preferring to acquire a delusive show of it from lectures, reading, and cram-tutors.

With the design of checking the tendency of the Licensing Bodies to underbid one another, after the manner of Dutch auction, for the favours of the idle and ignorant students, the Medical Council was established by the Bill of 1858. The members of it profess, and evidently have, a great anxiety to do their duty. They have appointed visitors to inspect examinations, and report to them the results, they collate these results, they complain of what seems to them defective, they correspond with examining authorities, and they give advice ; but very little appears to come of it. Hospital teachers still see at each examination season diplomas given to pupils of theirs, under whose care they would no more place a friend than they would ask a navvy to repair a watch. Doubtless the Medical Council also see and lament this, and have done all they can to remedy it. Therein lies the misfortune. We fear we must be content with their doing no more. The next step would be to present to the Privy Council incompetent boards of examiners, for the purpose of withdrawing their licensing powers—an important step, indeed, on which hangs the whole value of the action. But this step they will never take. They are too numerous to be united,

¹ See the 'British Medical Journal,' July 17th, 1869.

and without zealous union a large body can scarcely be moved at all ; or if moved, it is in such a laggard fashion that failure is inevitable. To enter upon such a warfare, without security of success, would be ruin, and we cannot wonder at their shrinking from it.

It seems agreed on all hands that there is little prospect of improvement without a change in the constitution of the council. The proposal made by Dr. Sibson's deputation is, that one fourth of its members should be elected directly from the whole body of practising medical men ; and another section of the British Medical Association (the Parliamentary Committee) advise that one half should be so elected.¹ The first-named gentlemen decline to make any suggestion as to the details of carrying out this scheme ; that is to say, whether it should be managed by increasing the numbers of the Council, or by excluding some who at present occupy seats. And we must say this reticence reduces much the value of their advice ; for it is on the practical working of the details that success in such matters depends. Statesmen have a great suspicion of the bare enunciation of principles, especially if they are accompanied by figures without reasons given for the selection of those figures. We have been in vain trying to find out how it is that the same principle should justify the demand for half which in other minds is satisfied with a quarter. We hope they are not adopting the undignified course of asking for an ell in order to get an inch. But the "Parliamentary Committee" are somewhat more definite ; they propose to reduce the numbers of the representatives of licensing corporations from seventeen to nine, and to fill with general representatives the eight seats thus vacated. The reduction to nine they would effect by grouping the elective bodies. Here again is an arithmetical conundrum. Supposing ourselves to be an absolute-constitution-maker, we have been trying our hands at its solution, and have had to give it up. While practising on the universities, as far as three or four seats we got on swimmingly enough ; but the residuum was chaos ; the sensation was like being shown a stud of elephants, cobs, and shelties, and being told to put them in pairs for double harness. The Medico-Political Association again propose to secure representation by conferring votes for a representative on the whole mass of licensed members or graduates of each corporation, instead of his being chosen by the governing body, as at present. This would keep intact the numbers, and probably the present respected members of the Council ; but it would also leave

¹ 'Report of Parliamentary Committee presented to the Annual Meeting at Leeds,' July 30th, 1869.

intact the whole defect in its constitution, merely adding the trouble of a canvass and the heart-burning of a contest. Besides it would bring in as voters the very persons whose incompetence we view as a disgrace—in some constituencies, perhaps, a fatal crowd of them.

The great objection to these plans is that none of them tend to the rendering the Council more efficient as an executive power. Any man's personal experience may convince him how impossible it is for two dozen gentlemen, even though they be the gravest specimens of the gravest profession, to meet together without wasting nine-tenths of their time in talk—a tendency which is further fostered by the presence of reporters. Of such an assembly the policy must be mere compromise, the enactments reluctant compliances with public opinion, always sluggish, often illusory; their vengeance against evil will be tardy, and their encouragement of good so faint as to be almost damnable. It is not big enough to be brave, nor small enough to be active. In fact, it cannot be executive at all. A reduction of the Council by at least one half is imperative.

Both the propositions we have discussed seem to be underlaid by a mistaken notion of the real political position of the Council. They look upon it as a sort of parliament for the representation of the various interests in the profession, and independent legislation on its internal affairs. Such a thing is impossible in a free state; it would be an *imperium in imperio* quite irreconcilable with safety and liberty. If it ever attracted any notice beyond a smile, it would have to be either dissolved or silenced, or stimulated to over-talk, after the manner of the country's dealings with a similar body, the Convocation of the Anglican Church.

The notion attempts to ground itself on the dogma that in a constitutional country all those who pay taxes should have a share of power in their application; and that since they pay registration-fees the registered should elect the Councillors who spend those fees. But it should be remarked, in answer to this, that they have already a controlling power through the House of Commons over *all* taxes, the Medical Registration tax among others. The Parliamentary Committee of the British Medical Association, in point of fact, acknowledge this when they speak of exerting their power of "directing the attention of some private members of the House of Commons"¹ to certain matters. The Medical Registration tax is part of the imperial revenue, which simply for convenience is diverted to pay the Councillors,

¹ Report of Parliamentary Committee, 'British Medical Journal,' August 28th, 1869.

and the expenses they may deem necessary. The payment of a special impost confers no separate claim to regulate its distribution. The squire who pays duty on a carriage has no more right to inquire what becomes of the money than a mere compound householder; and the man who buys a letter-stamp, or drinks a taxed glass of wine, would be laughed at if he claimed on that ground the privilege of directly electing the postman or the gauger. The Medical Council does not enforce a tax on its own authority, or even fix the amount. If it did so we should deem it an unconstitutional power calling for immediate suppression.

We are glad not to recognize the force of this argument for the direct representation of all registered practitioners in the Council, in addition to their indirect representation through Parliament, because it does away with the dilemma of considering how to choose a small consultative and executive assembly by competitive election. It is impossible that it should be done well, and unlikely that it would be done without seriously harmful results. Suppose only about a dozen candidates to be put forward for the six places, it is clear that none would have a chance without pledging himself to a particular policy; and it is needless to say that policy is more likely to be popular if it promotes the special advantage of the profession, regardless of the public wishes. What is to prevent members being returned under a promise that their chief business will be to put down homœopathy, hydropathy, restorative medicine, bone-setting, or whatever may happen to be the pet panic of the period? The unfortunate minority in such a small house would never get represented at all.

Besides recommending changes in the constitution of the Council, the deputation emanating from the British Medical Association have called the attention of the Government to the advantages which would accrue from concentrating the nineteen independent examining boards into one for each section of the kingdom, making this the sole portal for the primary admission of candidates. This would at once do away with that injurious competition in facility upon which we have commented. The spokesman shrewdly remarked that "the machinery for such a common examining board is already at hand;" and he shows, as by way of illustration, how the College of Physicians might appoint the examiners in medicine, therapeutics, forensic medicine, and physiology and anatomy bearing on internal disease; the College of Surgeons, the examiners in surgery and in descriptive anatomy; and the Society of Apothecaries those on the knowledge of drugs. A serious flaw in this suggestion is that the universities are left out in the cold, though the reports on the conduct of their communications show that they have discharged

the trust more efficiently than the corporations who would be favoured with the patronage.

The Medical Teachers' Association in their report "on the present obligatory conditions of study for the medical profession, and on the constitution and relations of medical schools," do not appear to feel themselves called upon to give any opinion as to the constitution of the Medical Council. They express themselves, however, as very much dissatisfied with the results of its action upon the examining bodies. To procure uniformity of qualification is stated by them to have been, "for longer than living memory the endeavour of all intelligent medical reformers, both primarily in order to a better conduct of medical education, and ulteriorly for the better fulfilment of our relations to the public. To promote its adoption was the main purpose for which the Medical Act of 1858, with its expensive consultative machinery, was advocated; and we think that our medical schools, no less than the general public, may reasonably complain that ten years after the passing of the Act the old chaos of rules and qualifications is still continuing as before." They urge as a remedy the harmonizing of the nineteen different boards by the enforcement of a code of regulations, according to which the examinations should be conducted.

Such a code would certainly be of some use in arranging more conveniently the mode of study, but it would not hit the blackest blot, that is, the testing of the results of that study. It would save the authorities of the school much useless trouble and annoyance, but it would not secure the public against the passing of incompetent students. It is very easy to instruct examiners to ask a certain kind of questions, but nobody can control their diverse valuation of the answers. It is very easy to insist that they shall require certificates of attendance at so many courses of so many lectures, and no more than the prescribed number; but it is very difficult to weigh those certificates, though it is well known that some mean only that the pupil has sauntered occasionally into the theatre, and others that he has been prepared on the subject to the full extent of his capacity.

Any system of testing qualifications which leans upon certificates of attendance at lectures is utterly fallacious. It is throwing on the teacher the office of bearing witness to the due diligence of his pupil; it is a shirking of duty committed to the examiners alone by the country. The University of Oxford has for some years led the van of reform by ceasing to demand them, and undertakes alone the work, as well as the responsibility, of finding out whether the candidate has studied his

subject or not. We are not without hopes of seeing the example followed.

The spokesman for the deputation from the British Medical Association to the President of the Privy Council acts, we think, judiciously in uniting the subjects we have just been discussing, namely, the reform of the Council and the establishment of one imperial test of capacity. He foresees their natural connection, and that one necessitates the other. Now in the plan which we would suggest the connection is drawn still closer, and indeed both goals must needs be aimed at simultaneously. We think that the success of both reforms would be more certain thus than if each had to run the risk of new wine in old bottles.

Let us not make the legislation for our serious and real profession a mere compromise—a conciliatory concession of part of powers or dignity demanded by pertinacious selfishness. Let it be grounded on principle and strict political justice, or let us not have it at all. Now what is the reason for the formation of the Medical Council? Every deliberative institution implies the existence of two parties or interests, between which it has to adjudicate. In the preamble to the Medical Act these parties are rightly pointed at as persons requiring medical aid, and the persons professing to guarantee it as of good quality—"Whereas it is expedient that persons requiring medical aid should be enabled to distinguish qualified from unqualified practitioners, be it enacted," &c. This simple principle must not be obscured by the importation of foreign colouring. Well, then, the interests to be represented are those of the public on the one hand, and those of the teachers, examiners, and licensers on the other. These latter are, in fact, the monopolist manufacturer of the raw material into useful implements; and if the implements are not so good as they profess to be, the employer has a right to ask the reason why, and to insist that the lucrative monopoly is conceded only on just economical grounds. We hold, then, that one half of the Council should represent non-professional interests. And we would apportion those interests thus: The Poor-Law Boards, as guardians of the most numerous and helpless of our countrymen, should return a member each, the Admiralty and Horse Guards as large employers a member each, and the Crown one more, representing the rest of the public. This last is the largest and most powerful constituency indeed, but, by virtue of that independent power, capable of defending its private interests individually, and not requiring so much care. To this we would add as chairman a statesman also nominated by the Crown, and six members elected by the Board of Examiners, the constitution of which Board we are just going to describe. These thirteen should be the Medical Council of

the United Kingdom, which could easily divide itself into Branch Councils for England, Ireland, and Scotland.

The Board of Examiners should consist of twenty medical men of each section of the Kingdom, viz. two examiners in medicine, two in surgery, two in midwifery, two in anatomy, two in botany, materia medica, and pharmacy, two in physiology, two in chemistry, two in forensic medicine, two in general physics, and two in preliminary education. Of these twenty, nine might be chosen by the Corporations and eleven by the Universities. From the Colleges of Physicians might come the two in medicine and one in midwifery; from the Colleges of Surgeons, two in surgery and one in anatomy; from the Societies of Apothecaries, two in pharmacy and one in midwifery. The Universities might take their turns in the election of the remaining eleven according to a rota. The examiners should hold office for short periods, but be capable of re-election, unless the Council interpose a veto, up to the age of sixty-five.

Each of the three boards of examiners should return two members to Council, whose salaries ought in all fairness to come out of the imperial exchequer, as it is, according to our view, an imperial concern in which the public have equal interest and voice with the profession. The fees for examination should be united into a common stock, to be divided among the examiners in proportion to the quantity of time spent by each. Candidates who are plucked should pay the same as those who pass for the examination, but to the latter should be added a small fee for registration.

The medical Council should have the power of visiting, of making, with the sanction of the Privy Council, regulations for the conduct of examinations, and of removing incompetent examiners.

To their present powers over the register should be added an autonomic right to erase names for the same offences as the present licensing bodies, and also the power of temporary suspension for scandalous conduct, such as habitual drunkenness, seduction or adultery.

They should have the power of establishing examinations for giving certificates to midwives and, perhaps, to the exercisers of some other offices of trust (such as keepers for lunatics); and persons should be liable to summary conviction before a magistrate for falsely pretending to have such a certificate.

To the medical Council should be delegated the duty of visiting schools and licensing them for a part of the needful curriculum of study, or for the whole, according to the facilities which they afford to students. The establishment of central schools of anatomy, chemistry, and some other sciences would be a great

relief to teachers and hospital authorities. The machinery for frequent visitation is at hand in the shape of the inspectors of anatomy, who might be made "inspectors of schools" and placed under the central Council. We confess we cannot sympathize with the lugubrious appeals of the teachers at St. Bartholomew's Hospital "to be left alone, and not troubled with incessant agitation;" nor can we accede to the opinion "that little good can be effected by any amount of legislation."¹ We believe that shaking up is very good for schools, and that a certain amount of legislative concentration of the present discordant requirements of the licensing bodies will save teachers frequent losses of temper and time.

Such a body as we have described might fairly be made a Council of Health, as well as of medical education and registration, and thus save the appointment of temporary special commissioners, who are naturally tempted to do their work in an amateur perfunctory style.

We should prefer the constitution of the medical Council being left as it is to any change which makes a large proportion of its members the sort of people who would canvass the mass of the profession for the honour of representing them. The dissatisfaction created would be enormous; for the failing minority, unable from the smallness of the numbers of the proposed representative to have their opinion reflected, would form a bitter opposition, ready to find fault with every proceeding of the elected assembly; while the successful party would be too weak to carry on any efficient executive measures under the constant fire of their opponents, reinforced as they would be by those to whom chaos is gain. Only, if the Council remains as it is, we hope its functions will be confined, as at present, to keeping the register, sanctioning the Pharmacopœia, and collecting information about education. We should deprecate its endowment with any extended powers. It was intended to be, and is, a temporary body, and as long as it lasts there is at all events the hope of its aiding in its own conversion into something better. But a new Council, with a false show of a popular basis, would be an "old man of the sea," from which we should find difficulties in ridding ourselves.

One most efficient safeguard against popular mistrust of the tests for diploma would be to make them public, at all events as far as the *vivâ voce* and demonstrative part. This would at once render impossible all those freaks of fancy and good jokes concerning the doings of examiners, which now render them

¹ Letters of Messrs. Holden, Callender and Savory, in 'Report of the Committee of Council on Professional Education.' (1869) pp. 67, 84.

ready butts for ridicule and suspicion. It would also take away from the candidate all plea of not knowing what to expect, and from the board all dread of false accusation of unfairness. During the examination for the medical degrees at Oxford the gallery is open to all comers; the privilege is often exercised, and never abused. Written work, indeed, is not shown, but no one can be plucked on that only. Let us have done with secrecy when we establish an imperial test for the license.

The second grievance we named of those endured by the medical profession is the competition of unlicensed practitioners. It costs us a great deal of money and much more time to acquire the knowledge which deserves a diploma, and it is certainly hard that our just gains should be intercepted by uneducated persons. This is a subject most difficult for a medical man to write calmly and judiciously about. He is feeling (oh, how acutely!) every day the pangs of ignorance; he knows, in spite of every exertion, how much his own unavoidable paucity of knowledge endangers his patient; and he can extend no pardon to one who dandles in his grasp the lives of his fellows without professing to have tried to gain that knowledge. He sees thousands losing their health and lives from the misuse of remedial appliances. He knows that the sale of drugs by persons ignorant of disease is scattering firebrands and death; and that the number of those who perish in consequence exceeds that which can be saved by human skill. He feels inclined to invoke the strong hand of law to interpose a wall instead of merely putting up a neglected danger signal. In point of fact there is a cry for power to the medical Council to put down quacks and unqualified counter practice.

But we must not shut our eyes to the fact, stated with such evident reluctance by Dr. Heslop, that want of confidence in medical practitioners, and the too high rate of their charges, are the reasons for the resorting of the labouring classes to unqualified persons during their own or their children's sickness. Confidence is a plant of slow growth, and we cannot expect to see the ripe fruit of our exertions in planting it; but we are sure that the adoption of the means we have recommended, namely, the raising of the standard of the minimum examination for diploma is the necessary first step in the right direction.

Here is a dilemma; for this enhancement of the goodness of the article involves an increased cost of production, and possibly a diminution in quantity, so that there is really a serious risk lest it should result in the price of medical advice being raised. And at the same moment to begin forcibly depriving the poor of what they have hitherto trusted in, spite of its badness, would be tyranny. Be it also remembered that those druggists and

bonesetters are not wilfully dishonest, like quacks, but only ignorant.

What is to be done?

To be just to the labouring man we must not pauperise him by establishing gratuitous dispensaries in greater number than at present; there are too many already. We must lay aside our dignity and supercilious almsgiving, miscalled charity, and compete with the unregistered on his own ground; we must offer the article at the same price, but genuine, that is to say, medicine unadulterated and advice worth having, for a sum he can afford—shall we say the eighth of a day's wages? That is about what the comfortable classes seem to think a fair charge; a family spending about three hundred a year do not complain of half-a-crown a visit, while a gentleman living at the rate of two thousand employs a medical man of a class that expects a guinea, if such a one is to be obtained in his neighbourhood, and runs up a bill at the druggist's as well. The eighth of a labourer's daily wages will hardly give more than three-pence a visit, including medicines; yet we are not without hopes that even that small sum might, by economical management, be made to pay remuneratively a medical man's labour, as well as to furnish drugs of good quality. For example, to go a little into details, a medical officer of a dispensary can prescribe without difficulty for eighty patients in four hours daily, six times a week—a much greater number pass through the out-patients' rooms of city hospitals,¹ and these latter contain also in their ranks more new patients than would be the case in fixed populations. This, on the lowest scale of charge would produce £310 a year, which might be expended thus:—

Medical Officers' Salary	.	.	£100
Dispenser	.	.	60
Occasional Servant	.	.	10
Rent	.	.	20
Drugs	.	.	120

However, as a matter of moral certainty, all the patients would not be from the families of unskilled labourers; several daily are sure to be of the servant or of the artizan class, who should be required to pay sixpence for each visit or supply of medicine. And where from this cause the income is larger, it will be right to make a proportionate increase of the sums above named. These calculations are exceedingly rough; but we were unwilling to go through the labour of elaborating, because, in

¹ At St. Bartholomew's Hospital, 100 casualties per hour are attended to by the house physician for five hours daily, in addition to his attendance on the physicians and care of the patients in the wards; but he finds that too much.—*British Medical Journal*, Sept. 17th, 1869.

truth, they will not weigh so much in ordinary minds as the self-evident fact that *druggists make such a business answer, and therefore so could others.*

These dispensaries should be carefully kept distinct from parish relief; even the appointment of the same medical officer should be avoided, and their inspection should be conducted by anybody rather than the Poor-Law Board. It might be done probably by the Medical Council, who, at all events, would be good trustees of the appointments. The selection for the junior posts should be by a competitive examination; but promotion to the more valuable incumbencies should be dependent on the way the work is done.

The succeeding subject is a nasty one—"How shall we deal with the Quacks?" being a question that often is brought before us in practice, and as often hustled out of the way for its unsavouriness. The use of a pretence of medical advice and science, for the purpose of extortion, by obscene advertisers and museum keepers, is annoying to medical men, and they are bound as good citizens to try and persuade the victims they meet with to publicly punish the swindlers. But there is a risk in coming too prominently forward, lest we should appear as if acting out of rivalry or prejudice. The sufferers also shrink from publicity. The temptation is strong to compound a felony here, and we must confess ourselves to having assisted a victim in that manner. The real want is that of a public prosecutor to carry on such cases, for we are not bound, as medical men, to act, any more than the House of Peers is bound to prosecute swindlers who assume titles. We must utterly repudiate all connection between quacks and doctors, especially the assumed connection of rivalry.

But the Queen, Lords, and Commons are respectable rivals that we need not be ashamed of. And when we find them opposing us by taking a profit on Patent Medicines, we think we ought to let them know they are doing a very dishonest thing. We look to the Council to do this very simple business.

To proceed to the next *gravamen*—we are disposed to think that many of the grievances experienced by Parochial Officers would be dissipated by the opening of a new career for medical men through the dispensaries we have advocated. Guardians will lose their grip on a struggling surgeon, when there are other appointments to be had besides that which they force on his necessities.

We have now gone through those points in medical politics in which the aid of the State in reformation may be fairly invoked for a trenchant remedy. Other cases there are where the changes must be gradual and continuous to be effectual, and

which, therefore, we had better be content to keep in our own hands. Such, for instance, is the METHOD OF EDUCATION adopted in our country. The lecturers and other teachers in the medical schools, though they foresee the necessity for a change in the system of licensing and in the constitution of the Council, are yet not deterred by their want of confidence in the present authorities from giving their attention to self-reform. Their "Association" has issued two Reports, and 131 separate teachers have, in answer to a circular issued by the Medical Council, written letters on the subject of their several departments, which are printed in the appendix to the Report of the Committee on Professional Education (1866); and we have also in the same volume the Report itself from a Special Committee of Council, several of the members of which are or have been teachers, sketches of several foreign schemes of education, a reprint of the above-named Association's reports, and a paper of 'Observations,' by Dr. Christison. To discuss all the innumerable details entered into in this heterogeneous collection of experience and views is, of course, impossible. And, moreover, it would be beside the purpose of this article, the intention of which is to handle the political relations of corporate masses, rather than the duties of individuals. There can, however, be no doubt that considerable improvement has, during the last dozen years, taken place in the quality of medical teaching; still less doubt that there is great room for zeal and for further progress in that direction. "Rest and be thankful" is seldom the motto of Anglo-Saxon workers.

What we are here concerned with is only what they propose to effect by the agency of State legislation, in alleviating the disadvantages enumerated in the first part of this article as weighing on our profession. In the first place, both the Committee of Medical Council, and all those whom they have consulted, are in complete accord in viewing the education of the lowest class of student as that which is of most importance to the profession and to the country. They feel that if they raise the average standard of the minimum qualification, the more ambitious may fairly be left to find their own way upwards. We are happy to say there is in medical instructors of youth none of that desire to turn out clever pupils, and to stimulate by emulation, which has unfavorably distinguished public schoolmasters. They do not do it, and they have no desire to do it.

The next point we would notice is, that wherever the subject is alluded to a uniformity of minimum qualification in those entering the profession is made an essential measure. The Council is advised by its committee that "the time has now arrived when, leaving to the universities and corporations full

liberty to deal as they please with their honorary distinctions and degrees, the Medical Council should endeavour to effect such combinations of the licensing bodies included in Schedule A, as may form a conjoint examining board for each division of the kingdom, before which every person who desires a license to practice should appear, and by which he should be examined on all subjects. Any higher degrees he may wish to take should come after, and should be optional. This plan is one which the Council has often approached and recommended in principle. We feel assured that the examinations for license will never be made satisfactory without it, and, therefore, that it is for the public good to enforce it without delay." If they are driven to herd together, the universities and corporations will do so amicably enough. But it will be necessary to have some scheme, such as we proposed earlier, for the assignment to different bodies of the nomination to different examining chairs. For the governing authorities of the several parties concerned are so diverse that their meeting to elect jointly the required board would be impossible. We trust the Council will be wise and bold enough to follow the recommendation of their committee, even though so doing should be equivalent to signing their own death-warrant. They will have done the temporary work for which they were created, and may repose on well-earned laurels.

As to the CONDUCT OF EXAMINATIONS, a needless multiplicity is to be deprecated, partly on account of the expense, partly on account of the interruption to the student's regular studies. We are glad, therefore, to read that the Council's committee think that two—one at the end of the second winter session, and the final at the end of the fourth year—are enough. We concede to the convenience of lecturers, examiners, and perhaps of students also, this subdivision of the testing process; but we must not pass over the warning of a certain danger which it introduces. Just as a reviewer, when he has once corrected his proof, wipes out of his mind all recollection of the authors he has been so familiar with, the student, when he has got his "testamur" for primary scientific subjects, is tempted to never think of them again. The anatomy and chemistry of a youth who loves them are, indeed, always cropping up in the wards and impregnating with double vigour the new seed being sown; but the average pupil is apt to be found to have put them on the shelf at the very time when they are most required to be made practically useful. It ought to be understood that at the final examination the candidate will be tested as to the degree to which he has kept up preliminary subjects. We have known an examiner arrested by a colleague at a final examination from proceeding with a question as being too "physiological" or "anatomical"

(we forget which). Now, we contend that such questions should be not only allowed, but enforced, at final examinations.

No candidate should be allowed to pass on verbal evidence only: his knowledge of chemistry must be tested in the laboratory, of anatomy by dissection, of medicine and surgery at the bedside. To the two former no objections, we believe, have ever been made; but to the last serious impediments are stated to exist. It is urged that the authorities of hospitals will not allow their wards to be used for such a purpose; and secondly, that it will be disliked by the patients. We apprehend that the first objection is entirely dependent on the second, and that if it is found not injurious or distasteful to the sick, no philanthropic body will prevent it out of a perverse sense of propriety. Now, as a matter of fact, poor patients do not raise objections; indeed, they seem to enjoy the excitement and *éclat* of being made subjects of inquiry. And as to the amount of extra exertion and pulling about which they would be made liable to, let it be remembered that these students are supposed to have been, for at least two years, daily in the wards observing the sick, taking cases, practising auscultation, &c., &c.; and surely it will not add to the risk of the patients to have their visitors there a few days longer at the same work, especially when it is carried on under the eye of an examiner. The great mass of the students are already being educated in the metropolis, so that very few additional need be introduced into the wards during the examination. Out-patients also could be made use of, and were it necessary to go further afield, they might be brought to the theatre from dispensaries at little or no expense. Two or three patients a head might be easily found. In point of fact, no difficulty in examining, clinically, for the licence is experienced by the London College of Physicians, or for medical degrees by the Universities of Oxford, Cambridge, and London, and there is no reason why it should not be generally enforced.

Examinations are usually conducted in too great a hurry. By this an advantage is given to the crammed candidate; he wants to fire off his prepared answers before they evaporate, and hopes that his failures in practical work may appear the accidental result of haste. Whereas the well-educated student is usually a slow thinker, and if dragged quickly from one subject to another rarely does himself justice. Now in England the longest time allotted to examining, even for the higher degrees, is a week, and at some Scotch Universities it is stated to be all over in a day. The opposite extreme is the "Staat's Examen" of North Germany, which is divided into "Stations" like a pilgrimage, and lasts over four months even to the most fortunate candidate. A medium may easily be

found, and in Britain it would be fixed probably much on this side of half way, partly from our dislike to sudden change, and partly on the more serious ground of economy. But we do not think students' means are so narrow that a fortnight's detention at the head quarters can injure them.

There are several subjects which have at one time been considered as necessary, at another as collateral, and again sometimes as merely ornamental to the study of medicine. We may specify general physics, botany, zoology, embryology, among the scientific matters, and insanity, special surgeries, climatology, mineral waters, among their practical applications, which come under this category. One is loath to discourage these studies even in the slowest and most plodding pupil. Indeed, the knowledge of logic and classification acquired in working up the first named may be the very best means of bringing his mind to a higher level; while a man's success in life may be sometimes secured by his knowledge of a speciality. At the same time, it is most important not to demand a superficial knowledge of too many things, a diluted omniscience, from the student. There is no immediate hurry about the matter, but we should be disposed to make such subjects voluntary, and to introduce them gradually and one at a time. The knowledge of them should be set to a student's credit and assist in passing him, but they should not be required. General physics, or botany, or both, might with advantage be made subjects for matriculation before entering on the study of the profession.

But on these extra subjects we should be disposed to leave a good deal to the discretion of schools and their teachers. Some may have opportunities and fancies to make a speciality of one subject and some of another, and we cannot call upon all to afford equal facilities for studying everything. Some minds also may be seen by their directors to be in want of that training which scientific classifications give; others need it not; and it would be unwise to bind all down to the same inflexible rule.

As regards also the REGULATION AND SUCCESSION OF STUDIES, we should be disposed to leave rather more to the judgment of different schools and hospital authorities than is generally contemplated by reformers. Plans may be suitable for a large school which are by no means the best for a small one; they each have their advantages, and should be given full liberty of exercising them. For example, as to clinical teaching, the familiar conversational bedside instruction, so easy and agreeable in the lesser schools, becomes impossible when a flock of fifty or sixty are following the professor; while, on the other hand, the brilliant demonstration of a lately visited case, such as M. Chomel used (*consule Planco*) to make so effective at the

Hôtel Dieu, is ridiculous before a class of half a dozen. Both are calculated to make a deep impression on the juvenile mind, but they must be accompanied by appropriate surroundings or they are failures. Again, in a small school, class-examinations cannot be conducted with any spirit, and will not produce the good anticipated by the committee of council; in a large school they are most efficient. While, on the other hand, the strict discipline under which alone the large number of youths can be admitted into hospital wards is not needed where there are only a few, and thus an earlier and more protracted study of disease at the bedside is both possible and desirable; and this cannot be done in a large school. Analogous remarks might be made on the modes of giving instruction in anatomy, chemistry, and so on. Liberty to teach in the mode they find most efficient should be left to each set of lecturers, provided always they show that they possess the appliances in the shape of museums, dissecting rooms, convenient hospitals, &c., which they implicitly engage to supply by the fact of admitting pupils and issuing a prospectus. This may be secured by a sufficient visitation.

To enable parents and guardians to form a just opinion of the value of the various educations afforded by different schools, an annual return should be printed of the numbers passed and rejected from each; and it would be well if a private communication of the grounds of rejection were made from the examiners to the lecturers. At the same time it is but fair that the school should be able to stop an idle pupil from presenting himself.

There is something almost childish in the faith which licensing corporations seem to have in certificates of attendance at lectures. What possible concern of theirs can it be how the candidate has acquired his knowledge? If the information were of use to any curious collector of educational statistics, he certainly would not gain it from these stupid "schedules." Both pupil and lecturer feel degraded when the time for the asking for and insertion of the signature comes round, and it is complied with as an annoying formality, utterly devoid of meaning. Surely it should be made the business of the schools to see that their pupils were industrious, and then they would do it.

English people are in the habit of considering Prussia a paternally governed country, where every serious action in life is gently regulated by an inflexible machinery; and truly there is a smoothness of motion in life there, so long as you are quite in harmony, and a discomfort when you get out of gear, that is a strong reminder of clockwork. But in the matter in question they allow much greater freedom to individuals than we do. The candidate for the national diploma

is never asked where, how, or in what order he got his knowledge. When he enters on his career of special instruction a paper of advice is indeed put into his hands, stating what the senate considers to be the best mode and order in which medical science can be studied. He is, however, by no means obliged to conform to it, and free scope is given to individual circumstances, and even whim; so that the young men rarely complete their education at one hospital or theatre, but migrate from one to another to hear the best teachers on special subjects.

In France the place of "schedules" is taken by a great frequency of examinations, the order of which binds the pupil, of course, to follow the same in his studies. We have already given reasons for objecting to this plan, but at all events it exempts the lecturer from pricking in his class like a schoolmaster.

The reality of the final examination for license is the only thing the public can and will trust to, and we are most loath to dilute or disguise its prime importance by appending other tests.

There is a subject of very inferior moment to any we have hitherto discussed, and which can be taken up as a relaxation from graver thought. It is the question of MEDICAL TITLES. There is a general confusion of the distinctions they were intended to make, and really all that can be supposed to be meant by the combinations of letters prefixed or affixed to a practitioner's name is that he is duly licensed by somebody. It is impossible from them to know if the person is a general practitioner or consultant, a physician or a surgeon, an accoucheur or a dentist, a male or a female. By common consent the title of "Doctor" is made generic; for instance, in the prospectus of a Dublin hospital, "the two physicians and the four surgeons are each set down as Dr., while only one of them—a surgeon, by the way—has an M.D. university degree" ('Mapother Prize Essay,' p. 218). This, of course, could have been done only with the consent of the gentlemen concerned. But, protest against it as he will, Podalirius or Machaon will be equally called "doctor" by the million, whether he presides in the senate house in a red gown, or makes up "doctor's stuff" at so much per bottle. In fact, the former is described as "a" doctor, whereas the latter is "the" doctor *par excellence*. The thing is done—there is no appeal; for in reality a man's title is not what he calls himself, but what other people choose to call him. And now, on the occasion of fixing a common portal, for instance, into the profession, there seems an opportunity of gracefully yielding and legalising by statute the established nomenclature. But our legislators cannot be expected to take the invidious initiative in this matter. We should ourselves petition for the authorised confirmation of the popular

voice, and pronounce ourselves ready to give up the empty shell to be filled with new fruit. "Doctor," in common parlance, "Medical Doctor," or M.D., in legal phraseology, would then be expressive of a fact, just as "Reverend" is expressive of a fact prefixed to a minister of religion. Not even an etymologist could object, for it is as much the speciality of our profession to be "*doctores*" or teachers, as it is the special duty of clergymen to lead people in the right path by the *reverence* paid to their character.

We believe there are some who would represent themselves as robbed of a prestige conferred by the university doctorate, for which they have paid in time and money, if others were admitted on easier terms. We cannot view the matter in this light. In the first place we would not make the terms easier, as regards what the title professes to guarantee, namely, the intellectual qualifications of the bearer. And, secondly, it must be remarked that the mischief is already done, the prestige is gone, and can never be restored by statute. There are eccentric persons who derive a melancholy satisfaction from reading in Dod that M.D.'s take precedence of Queen's Counsel, and who tell their wives that they are entitled to go in to dinner before the squire's or the rector's lady. For our part, instead of boasting thereof, we must say that to us it is rather a matter of shame to have lost in common repute such precedence; and instead of asserting our mediæval position we feel disposed to take silently the lower seat till such time as society should volunteer a gracious "Friend, go up higher."

A very strong argument in favour of distributing the title Dr. upon all practitioners is that no other suitable substitute has been suggested. "Apothecary" has too strong an odour of the shop. "Surgeon" is etymologically incorrect, and our friends in Lincoln's Inn Fields might cry Stop thief! Nobody ever heard of a "Licentiate" since he read *Gil Blas*, and it does not feel to fit with the English tongue. We doubt if an Act of Parliament could force it into common usage, and expect that many a bishop would have to be run over in New Palace Yard before the newspaper announced that "Licentiate M or N was immediately in attendance." We cannot think of any other title than Dr., but are open to proposals.

The stand made by many on professional titles is probably not all mere pedantry, nor to be disposed of by such light fun as Kotzebue's good-natured satire. There would seem to be at bottom a notion that our social status is somehow assisted by their integrity and maintenance. This imports a seriousness into a frivolous matter, if the notion is grounded on reality. But in good truth we do not think it is, and what is more,

neither do we see any likelihood of our friends gaining the assistance they repine for from anything else.

This social status is a sore point. There is not a single one of us so philosophical but will find, on searching his own memory, that either personally, or through his family, he has been annoyed by being ranged below men in other positions, who legally, intellectually, by birth, by age, and by income, are inferior to himself. It is very obvious that in the opinion of little Frivola Bat's-eyes old Lady Vain-Pomps and Mr. Worldly Wiseman, while he would be considered rather to have raised himself by becoming a clergyman, or a soldier, or a barrister, he is thought to have lost caste by becoming a physician. Let not any prejudiced politician ascribe this to the aristocratic constitution of our country. Dr. Gross displays the existence of an even worse state of things in the American navy, where the young physician is "exposed to impertinence and insolence, if not to positive indignity," and can get no relief from Congress from a position worse than the naval officer of any European government.¹ It is of no use denying it, we *do* care, and our wives and daughters care more than we do. But it is to be feared that there is no possibility of comforting our junior brethren with an anticipation of this state of things improving in their time, or indeed at any time at all. We believe it rests not on accidental convention, but on an innate sentiment of human nature, and so is not likely to be altered. Broad-minded men, like Homer, have at any time during the last 3000 years been ready to agree that a medical man is really *πολλῶν ἀντάξιος ἄλλων*, without in the slightest degree influencing the popular voice, which reverses the valuation. This cannot depend on individual whim, there must be a general law governing opinion for so long a period. And the law we believe to be, that every occupation which brings the exerciser into personal contact with the body of another, is considered to be thereby lowered in rank. Take, for example, service: the domestic, though better fed, better clothed, and better mannered, is looked down upon by the lowest mill hand. An attorney's clerk, at sixteen shillings a week, despises the most dignified *valet-de-chambre*, whose place may be worth two hundred a year. Again, in the class of handicraftsmen, the barber, with all his smartness, is considered a contemptible personage in comparison of the cabinet-maker or blacksmith. Among tradesmen, the master tailor, though he drives his pair, is of less account than the brewer, though he may collect his bills in a gig. To wed a man enriched by selling shoes would be thought by a young lady

¹ Address to American Medical Association, page 10.

more of a misalliance than to wed a man enriched by selling ships. To go higher, the painter, who has been aiming unsuccessfully at historical high art, or even at *genre*, thinks he is condescending when he takes portraits. Even in the vulgar estimate of the ruling classes, traces of the same sentiment are to be seen; an hereditary precedes an elected sovereign, and a member of the house of peers is set above a representative of the commons, for no reason except that what one was born to, the other has gained by bringing his merits into contact with his fellow-men. This is not a mere conventional, modern classification of orders of men, but close upon that adopted by Aristotle in the 'Politica,' as suited to a very different regime. The same instinct introduced into natural religion has caused peculiar honour to attach to celibacy, peculiar disgrace to promiscuous intercourse, and this quite irrespective of any idea of criminality. Now, of all honorable occupations, there is none which brings a man oftener and closer into contact with his kind than ours; he is familiar with them inside and outside, mind and body. This familiarity does not indeed overbalance, but heavily counterweights, the respect felt for our moral and intellectual elevation. It cannot but be seen that nothing but the necessity under which we lie of having the best possible mental endowments has kept us from being pushed down altogether into a lower class by heralds of social rank. From the past let us take a hint for the future, and learn that we do more to hold our own place in society, by persistently enforcing the highest liberal education on all who enter our profession, than by any assertion of title, or appeal to historians and masters of ceremonies in favour of antiquated privileges.

Titular distinction, as between ourselves, is another question. Consultants will always, in communities large enough to allow of division of labour, form a class apart from general practitioners. And the former will always be divided into physicians, surgeons, and accoucheurs. But there is no necessity for marking that in the ordinary nomenclature. No distinction is made in that very subdivided profession, the law. A queen's counsel and an Old Bailey attorney are both of them equally "Mr." and "Lawyer," without any confusion. Nor is an equity draughtsman to be known by the 'Court Directory' from a pleader. The attempt in France to designate our grades as "Gradés," and "Officiers de santé," is a failure; for the second qualification is held by none but village druggists and a few foreigners who have already degrees. All decent general practitioners are graduates throughout the empire. A consultant is, in point of fact, a man who is consulted by his professional brethren; and he attains to that position of responsibility either

by being a teacher, a hospital officer of senior position, or by having become known through published researches on special subjects; very rarely indeed, but occasionally, by the accident of royal patronage. All this implies personal notoriety, which does away with the further need of any title by which to denote the department of practice peculiar to the individual. Let us remember that every consultant is a general practitioner as well; he never resigns his right to take sole charge of patients of all sorts; and until he does resign that right, there can be no claim set up for drawing a hard-and-fast line between the departments of the profession, as in the law.

The amount of REMUNERATION for private practice cannot, in our opinion, ever be made with advantage a subject of central agitation. It must always be regulated by the natural laws of supply and demand. And as the supply and demand vary in different parts of the country and at different times, so must vary the customary rate of payment. To justify a rate of charge in a court of justice, a practitioner has only to exhibit his account books, showing what his usual pay by the hour, by visit, or by mile, has been, and so much will be allowed. At the same time, it is open to local ethical associations to publish a scale of remuneration for their district, and this, if revised from time to time, would be received as evidence of common usage.

In the case, however, of salaries for public work we think it is the bounden duty of medical councillors to interfere. They should fix a minimum, below which they will refuse to sanction an engagement with parochial or other paymasters, just as bishops do in case of curacies, and they should obtain the power of suspending the license of any one infringing the rule. Let it not be supposed that we are advocating this measure to uphold the dignity of the profession. Our object is solely to get justice for needy and defenceless men. The scale of pay under the Poor-law is so inadequate that posts are accepted solely as introductions to practice, or to keep out a rival; and this should not be.

While on the question of money we would notice (though it is rather beside the subject of "Medical politics") that practitioners, especially consulting physicians, have got some customs as to the refusal of fees which appear to us unsuited to the present era. What valid reason now-a-days is there for not taking money from relatives, wealthy members of our profession, and the like? It makes the public think we do not deserve our pay, and is a source of embarrassment to the recipients of our kindness. If there is one greater nuisance than sending a present under these circumstances it is the receiving one, and the consequence

is that a sick man will often avoid consulting one whom he would prefer, because their families are connected, and that medical men will conceal their profession, and seek the advice of a distant stranger about their ailments. A man of much mark cannot do so; he has to say, like the poor giant in Seneca's play, "*ubique notus perdidit exilii locum*;" but we have experienced several instances of country doctors acting in this way to avoid the awkwardness of not paying for advice. There seems to us a vulgar, priggish affectation in refusing a fee when offered from anybody that can afford to pay it. The custom is never observed by the heads of any other profession, and we think our leaders ought to set an example of discouraging it.

When there is established a national examination for license, the question of the ADMISSION OF WOMEN to it will necessarily come on for discussion. We should be inclined not to exclude them. Our reason is by no means any speculative notion about the physical and moral equality of the sexes, but on the contrary a complete submission to the logic of fact that they are unequal. They are so decidedly unequal that we are sure men have nothing to fear from the competition, and if more than two or three women from time to time enter into or stay in the profession, it will be because they find out some hitherto unfilled place to which they discover an adaptability. We ought, as a point of political justice, to let them try to find this, for it would be an addition to the resources of the country, and a sphere of useful occupation. When we speak of inequality we refer, not to feminine capacity for holding the reins of government, voting, or any other power on which Mr. Mill has enlarged, but simply to the matter before us, namely, the probable utility of the sex as medical practitioners. Since the creation women have been burdened with one disadvantage which nobody can deny, and which will always make them unsuccessful as our rivals, a liability to marriage. If they are actually wedded, their patients will feel there is a strong probability of losing their services for upwards of two months annually; if they are still single, parents will hesitate to have such clever, pleasing, active young ladies, as we hope they would be, in and out at all times among their rising Strephons. Independent of this, mere deficiency of physical strength will render them unequal to the rough work of the profession, or to the continuous strain on the faculties, which the easiest-going of us have at some period of our lives undergone. Attempts to pick and choose their path must result in their being, as a mass, left behind in the race. But they will never be convinced of this unless they try. So we say let them try, and then, if they can find a way of making their peculiarities useful, we shall rejoice; if not, they will "fall out,"

like tired soldiers on a march, and be forgotten. We trust that none will be led to try the experiment without having resources to fall back upon in case of failure. There would be great cruelty in holding out the inducements of scholarships, cheap education, and so on, to poor sanguine girls. They ought to understand that the practitioners they view getting a livelihood are the strong and lucky only; they see nothing of the unsuccessful who have fallen back on trade, the church, the police force, the betting ring, farming, innkeeping, and other occupations of which we cannot at this moment remember instances; they ought to know that a large number, alas! melt away into utter obscurity. They ought to thoroughly appreciate their position, and then try their chance fairly.

The objections to the employment of ladies on the score of indelicacy we do not think can be urged by medical men, who are in the constant habit of seeing female nurses in attendance on both sexes. The difficulties in the way of education are more serious. The schools will not admit them; for the parents of students naturally demur to a batch of lively girls being turned loose among their boys. But if they have energy and capital sufficient to form separate classes, and to get opportunities of clinical study, they deserve the rewards of perseverance.

The question of SECTARIANISM is one that frequently rises up in all politics, medical, clerical, and general; and it appears to us capable of the same treatment in all. From the earliest ages clever, zealous, narrow-minded men, have been springing up from time to time, whose mental vision has been so strongly affected by some one colour in the compound light of truth, that they are blind to the rest. They proclaim aloud that to be lighted by a thus tinted ray is the only chance of seeing the path to cure, to salvation, to perfect government, as the case may be. Multitudes who had hitherto walked in darkness join them, find the half light good, and denounce as still benighted those who do not see things of the same hue. So is a Sect formed, an Hæresy, or Party, an *αἵρεσις*, or wilful "choice" of some "*part*" of truth instead of the whole. Let it alone, and it does its work of calling attention to something hitherto neglected, and then melts back again into the catholic brotherhood of seekers after the right. But persecute it, and it enlists in its defence courage, justice, perseverance, and immediately becomes a power. Being the weaker party it is easily frightened, and driven into overt acts of exclusiveness and other forms of persecution on its side, just as animals fly at you and bite out of mere terror. A striking example is presented by the history of homœopathy. Up to a few years ago it has been pursued with noisy threats, answering closely to the picketing

and rattening of the trades' unions. The consequence is that a number of medical men, whose opinions are by no means agreed on the truth of Hahnemann's various dogmas, have united on the common ground of calling pills "globules," and using a peculiar nomenclature and some peculiar drugs generally thought to be inert. This ingenious freemasonry shuts out others from critical consultation, and acts like a military uniform in banding them together as an exclusive sect. It is as valuable to them as a party cry, or the quaker's dress and speech; it has kept up as a visible body of men that which by this time would otherwise have merely remained as individual opinion, or modified (for good perhaps) some of our views of nature. If any one doubts this, let him observe what has happened in our day to hydropathy and kinesipathy. Their first "promoters" (as the commercial phrase runs) began by denouncing the doctors; loud were the trumpets, and red was the rag by which they may naturally have expected to get a run out of the bull. But their pretensions have never been counterblasted by any joint protest, or by any physician of note or conspicuous man of science;¹ luckily these were either otherwise engaged or careless, it is not clear which; but, at all events, they behaved as the world did to George Primrose's paradoxes ("And what did the world say to your paradoxes?"—"The world, sir, said nothing to my paradoxes.") The result of this treatment is that the practitioners of hydropathy keep excellent boarding-houses in the neighbourhood of fine air and pure water and prime mutton, where those that want them can have convenient baths, and those that don't want baths can still enjoy the above hygienic remedies with regular early hours and a sedative sort of society. They supply what was formerly a want in England. They have also taught the people the habitual use of water; so that, when we desire to employ that most valuable article of the British pharmacopœia, we have less prejudices to contend with. The kinesipathists have dropped into comparative obscurity, because, in truth, they were little wanted. English people seldom require to be taught the use of their limbs; but yet if they should, there are kinesipathic rooms and institutions ready at hand. Now, is it not certain that denouncing loudly even what was evil in these "pathies" might have made them, nay might still make them, into a system, and their promoters into a sect any day? They have

¹ An exception might be made of Baron Liebig, but he is not, strictly speaking, one of us, and seems to have been specially goaded by the individual insult of being claimed as a supporter, when he denounced hydropathic institutions as "dens of covetous and rapacious gamblers, where the wretched invalid resorts to throw dice for health and life." This is foolish language.

in them as many elements of truths as the dogmas of Hahnemann can have, and might exhibit more striking and immediate evidences of those truths; yet they have not prevailed over the abiders by catholic reason, nor have they any bond of union.

Perhaps even more striking still is the history of eclecticism. A few physicians banded themselves together on the principle of using no drugs except those prepared in a peculiar way (by the precipitation of a resin with water from its alcoholic solution) and they took the name of eclectics.¹ It appears that in the United States they got bullied; and the consequence is that the "eclecticism" is there a "system," has started a college, gives diplomas, and what is still more to be lamented, afflicts the soul of worthy Dr. Gross, who mentions them in his Address among other "outgrowths," which he penitently accepts as just judgments.² In England, we calmly adopt such of their drugs as suit our purposes and seem worth experimenting upon, and no more is heard of the sect. If they do not choose to use our drugs in return we are sorry for it, the loss is theirs, but we do not attempt to force them.

We need scarcely say, then, how earnestly we should deprecate authoritative or concerted action against any "system" of therapeutics. The spread of opinion must be left to the regulation of a higher power than ours—

Ist's Gottes Wirk, so wird es stehn,
Ist Mannes, es wird untergehn.

If our individual opinion differs from another man's individual opinion, let us say so openly; but let us not band together to put him down, or we become sectarians ourselves, and as such merit to be classed together under some opprobrious epithet. At present, emphatically, we do not merit such treatment; to pin the name "allopath" on our backs is mere vulgar scurrility; it is impossible to find any idea answering to that word in our teaching or practice; it is one of the *nomina sine rebus* of Bacon, the use of which can indicate only thoughtlessness or dishonesty in a writer. The adoption of any principle

¹ We are quite serious. Some time ago we inquired of an accomplished pharmacist, "Why are Podophyllin and Cimicifugin called *eclectic* remedies?" and the answer was, "We import them from So-and-so, of Philadelphia; there is no other reason." We thought we had caught our friend perpetrating a joke, but further inquiry showed it to be a sober earnest.

² "The cause of our woe lies at our own doors, in the imperfect appreciation and discharge of our duties. * * * Practitioner and professor, private preceptor and college teacher, are, in the existing state of this grave question, equally at fault and equally inexcusable. Every one may justly exclaim, in the language of the devout publican, 'God have mercy on me, a sinner'" (page 6). We can appreciate, though we cannot equal, this humility.

grounded on such partial truths would be especially injurious to medical science in the present day, when so much knowledge is fermenting around us and developing new growths of intellectual activity. For us to have an established theory of therapeutics, would be like adding an excess of alcohol or sugar to the wine-vat, or boiling the seeds we are sowing; it would arrest all further progress, and leave us indeed practising an art, but without any claim ever to erect it into a science. An union of any kind to put down "heterodoxy" would infallibly lead us to our having an "orthodoxy" of our own, and to all the just ridicule and contempt consequent on such a *damnosa hereditas*. We, therefore, quite agree with the prudent statesmen who limited the power of the medical council to erase names from the register by the clause, "but not on account of having adopted any particular theory of medicine."

HOSPITAL REFORM (meaning here, reform of the constitution, not of the building) has occupied the attention of a good many correspondents to periodicals, and is one of the subjects proposed for consideration in the programme of the Medico-political Association. The hardships which it is charged the existing arrangements of these institutions inflict upon our medical brethren are the following:—First, the profession is defrauded of its fair gains by patients, who could afford to pay, being treated gratis; secondly, posts on the staff, directly valuable in a lucrative point of view, or indirectly as openings to advancement, and which might profit a large number, are monopolised in the hands of a few. Other charges there are, but those which do not resolve themselves into the above-named are either of a very sentimental character or mere dirt-throwing.

The first evil is so constantly inflicting annoyance on those whose lot leads them to practise among artisans and small tradesmen, that they are very sore about it. Some are even driven to call upon the strong arm of power to shut up the present voluntary, self-governed hospitals altogether, by establishing and endowing others under the immediate control of the State in sufficient numbers to render the existing charities superfluous. They fancy they would thus get rid of abuses. Vain hope! Let them visit the state-managed hospitals of Paris, Florence, Naples, Rome, &c., and they will find the identical abuse complained of there displayed in magnified proportions. They will find often and often even the beds occupied by a class of persons who, in England, would never think of asking charity—students, artists, tradesmen, governesses, &c., to say nothing of the out-patient department. Our own belief is, that the abuse is in England not only less than under centralising governments, but less than it was among ourselves a short while

back. Not many years ago it was the custom for consulting physicians, and even some wealthy general practitioners, to appoint certain hours on certain days in the week, during which they prescribed at their own houses gratis for all comers. Of course the butler took care that those admitted should not be dirty or odorous paupers; and, as far as one can learn from biographies, where the sacrifice is recorded as meritorious, or from the memories of old persons, the patients were the very classes whose gratuitous medical treatment is now complained of. The extension of out-patient departments at hospitals and dispensaries has led to this custom being given up, and to the numbers of those who thus abuse the good nature of our profession being much diminished.¹ Besides which what remains is public, and it is not now impossible to learn the real extent of the evil.

It is not impossible to learn the real extent of the evil, but it is very difficult. To inquire into each patient's pecuniary circumstances, to estimate the truth of his statements, to weigh special exceptions, and to form a decision on the whole, after hearing the arguments on the other side, backed up by long-winded friends, takes at least twenty times as long as prescribing for him. To form an opinion by dress, manner, or general aspect is grossly unjust—one might as well judge by their smell. The class considered as proper to receive the benefits is very ill-defined; and what definition there exists is mostly negative. They are *not* to be paupers, and *not* to be capable of paying. This adds much to the difficulty of selection, and it also gives an unfair advantage to the denouncers of hospitals; for they are able to make the inaccessibility of reliable statistics an excuse for citing individual instances as if they were really typical. As to the cure of the evil, we think the most important step is the acknowledging it to be a real one, which is now pretty generally done; and the next is the temperate agitation of the matter among the governors of hospitals, whose interests as well as their benevolence are concerned in their alms not being misapplied. The Medico-political Association will not, we trust, take amiss a warning to avoid all exaggeration, lest their zeal should injure the cause; and in the application of rules we would suggest that they should not be too suddenly drawn tight, lest it should lead to the reintroduction of the more injurious custom of the last generation.

The limitation to a few of the honours and opportunities afforded by hospital appointments, we fear, is inevitable. Even now many highly gifted men hold them without attaining thereby

¹ This is especially marked in the case of specialities. Only the other day, on the appointment of an aural surgeon to St. Mary's, he closed a private gratuitous institution which was in his hands.

to any worldly advantage beyond constant occupation ; and if their number were much increased, they would cease altogether to be portals to fame. Other less creditable portals would be found, and the profession receive a severe blow. Doubtless it is annoying to see our neighbours luckier than ourselves, but it should be remembered that even in heaven all cannot be arch-angels.

Rather than let hospital reform degenerate into a contention between the "ins" and the "outs," it will be wiser for general practitioners to take a perfectly independent line. In large towns the pay-dispensary scheme, roughly sketched out a few pages back, would offer a sphere of local fame and usefulness. In the country a much better institution is already in operation, and working its modest way—the Cottage Hospital.¹ The pen is with a struggle withheld from pointing out the boons herein offered to the rural peasantry ; it dare not begin, for it could not soon stop ; and its business now is with the medical man only. No plan could have been devised more sure to raise his social status. Everybody knows that the foundation on which almost always a rising doctor's local reputation is built up are serious injuries and great operations. The all but impossibility of treating these in poor homes has led to the practice of transferring the worst cases to the nearest county hospital, in mere justice to the patient ; and thus a chance of honorably showing his skill is lost to the surgeon. We may hope now for a remedy against this loss ; in the cottage hospital there is no reason why the rural practitioner should not undertake those responsibilities which he has hitherto handed over to others. "Want of skill" may be whispered, but skill will quickly come with experience ; and even at first the fresh air, the visits of friendly faces, the avoidance of the terrible journey to town and of the painful sights in the city wards, may well compensate the patients for not being attended by the great man. With a cottage hospital at hand, we do not see that the provincial has anything to envy his metropolitan brother : he has every requisite for professional success, and much greater facilities for being known among the gentry and middle classes. His fame may not be so brilliant, but it has the advantage of near neighbourhood : a blue-bottle in the ear is louder than a lion in the jungle.

The subject of STATE MEDICINE we think it would be wisest not to press at present. In such a country as England it is better to let the cry for the doctor be a little louder before he

¹ "Cottage Hospitals: their Objects, Advantages and Management." By E. J. Waring, M.D. London, 1867.

brings out his instruments. What State Medicine is, or rather should be, for it has to be made, we will discuss in a future year.

Of "Medical Politics," as a whole, we are disposed to take a view cheerfuller than that of Dr. Gross. We have pointed out a good many holes in our coat, it is true, but most of them seem in a very fair way to be mended, and all are much smaller than they were a short while back. Inevitable disadvantages of our position there are some, but we are sure they are easiest borne when acknowledged, and seem to be inseparable from human nature as at present constituted. The increasing custom of meeting for friendly discussion, consolidation of our interests, and publication of real or supposed grievances, is more likely to amend evils than any amount of state interference. We would invoke that only in cases where it already presses on us unfairly. It is this feeling which led us to insert among the pamphlets for review one written under another form of government, in many respects differently interested and differently influenced from ours. In spite of this diversity its relations to the profession seem very similar to what exist here, much more similar than is the case in any European country, empire, monarchy, or republic. Why should there not be an association of associations, which should show the world how much warmer blood is than water? We have watched the noses of American citizens pleasantly buried in Oxford tankards, and we would urge our readers to return the compliment at the earliest opportunity to the American Medical Association. Those who cannot do so in the flesh are still not chained in the spirit; kindly feelings, like kings, have long arms; let us stretch ours across the cannon-guarded walls of states, "*ultra flammantia mœnia mundi*," across the water which "cannot quench love," and take by the hand those ancients Britons who are marching the same way as ourselves.

II.—On Entozoa.¹

THE larger of Dr. Cobbold's two volumes had five-and-twenty pages of an 'Analytical and Critical Review' devoted to a notice of its merits in the April number of this periodical for 1865; and as the two volumes are parts of one whole we believe their

¹ *Entozoa: being a Supplement to the Study of Helminthology.* By T. SPENCER COBBOLD, M.D., F.R.S., Correspondent of the Academy of Sciences of Philadelphia. London.

learned author will have no just cause of complaint against us if we on the present occasion allot a much smaller amount of space to this, which is much the smaller of his two volumes. We will begin our notice of the second or supplementary volume by saying that the author has, in the composition and arrangements of his two handsome octavos, shown that he has thoroughly mastered the lesson which Nature, by omission as well as by performance, has so clearly impressed upon us in her construction of Vermes. A single volume may by itself be taken as corresponding to a single segment, or to what Mr. Herbert Spencer would call a secondary aggregate, in one of the Vermes. By the "specialisation," to continue the use of modern zoological language, of certain parts and pages so as to form indices and bibliographies, and appending them terminally, "individuation" is conferred on the composed organism, which becomes then a single and indivisible "tertiary aggregate." In other words, but with the same metaphor present to our minds, we may say that the second volume now before us, by possessing the useful appendices just mentioned, has greatly increased the availability for use of the first, and comes thus to be a necessary complement of, and not merely a supplement to, it.

The second volume begins with a history of the discovery of *Trichina spiralis*, in which we have an appraisalment and "ponderation," as Mr. Lowe might call it, of the relative claims of the various individuals who have contributed to our knowledge of this parasite, from Mr. Paget who, "it is clear" to Dr. Cobbold (p. 8), "first discovered the worm," Feb., 1835, down to Dr. Zenker, who "was the first to demonstrate" (1860) "that these parasites were capable of giving rise to a violent disease in the human body."

As Mr. Paget can very well afford to be robbed of a scientific discovery or two, and as any person who wishes to learn the real state of the case in this "*Prioritäts streit*," as the Germans call it, can do so by turning to Mr. Paget's letter, at page 269 of the first volume of the 'Lancet' for 1866, we forbear to fill up our allotted limits with names and dates. Still, there is a passage in Mr. Paget's letter which, as Dr. Cobbold has not reproduced it, as it transports us back into days which seem to us now to have belonged to the infancy of science, and as it puts us, at all events, into the company of interesting personages, we feel inclined to quote. It runs thus:

"The man in whom the trichina was first observed died at the hospital on January 30th, 1835. Three days later the dissection of the body was commenced. The report soon ran through the dissecting room that there was another body with spiculæ of bone in the muscles. Examining some of these "spiculæ" with a lens, I soon

found that they were cysts, and almost directly afterwards ascertained that nearly every cyst contained a small worm coiled up. I was anxious to observe them with a microscope; and possessing none I applied to the only man of science whom I at that time knew in London, Mr. Children, Principal Keeper of the Natural History Collection at the British Museum. He, I think, had no microscope, and he, therefore, took me to Mr. Robert Brown; and I shall not soon forget the feeling approaching to awe when I went to one whom I had long looked upon as the first physiological botanist of his time. I remember that when Mr. Children entered his room, he said: 'Brown, do you know anything about intestinal worms?' and the answer was, 'No, thank heaven! nothing whatever.' Mr. Brown at once lent me his simple dissecting microscope, with which I soon observed structures in the worm which were before invisible. He himself dexterously pulled a worm from the cyst, and I believe I still possess my sketch of it. As soon as the discovery of the entozoa was made known in our dissecting room portions of muscles were distributed far and wide."

In a subsequent part, however, of Mr. Paget's letter, in which he speaks of a paper which was read subsequently to his own as "an admirable memoir, much more complete and exact in zoological detail than anything he could have written," we must demur, as we find Leuckart (at page 510 of the second part of his second volume on 'Human Parasites,' which appeared last year), speaking of this very memoir as being as little exhaustive as possible, and in parts blundering, as the posterior part of the body was taken for the anterior, and the internal organization was entirely misconceived. This remark of Leuckart's may be set side by side with that which Pagenstecher ('Die Trichinen,' p. 6) makes as to the author of the article 'Entozoa,' in Todd's 'Cyclopædia of Anatomy and Physiology,' suppressing Paget's name altogether, and styling himself there the discoverer of the *Trichina spiralis*; and the profitable moral may be drawn that sometimes and in some places Nemesis does follow injustice even in the world. We say "sometimes and in some places," for we have been repeatedly assured by several persons of atrabilious, and several also of gouty temperament, that this is by no means always the case; and one of the cross-grained individuals we allude to avers that a man's merits are ordinarily in the inverse ratio of his deserts. We recommend this statement to the consideration of the Comtists, who would fain have us substitute the enjoyment of posthumous reputation for the old notions of heaven and immortality. "Conceive," says one of our friends just alluded to, "of a heaven attained by the acquisition, of an immortality consisting in the enjoyment of *stolen goods*!"

Dr. Cobbold has, in some three or four of his chapters, given us

accounts of detailed experiments on the raising and rearing of the various morbid and lethifical larvæ, with the migrations of which we are so intimately concerned. It is impossible to give an adequate account of these experiments here and now, or to do more than make some very general remarks as to their bearings. Of these general remarks, the most important is that the science of helminthology is, now that it has, before its past history is wholly forgotten, attained to the verificatory and experimental stage, especially instructive, in the way of warning us against over-hasty generalisation. We have now, by actual experiments with dogs on the one side and sheep on the other, had it positively demonstrated—firstly, that the many-headed “bladder-worm” from the brain of the sheep affected with the disease known as “staggers” or “gid” will, if swallowed by a dog, become in his intestine an army of “tapeworms” drawn up in columns; and secondly that one of these tapeworms, or indeed one segment or “proglottis” of one of these tapeworms will in its turn, if administered to a sheep, produce in it a greater or smaller number of “bladder-worms”; and we can now, if we be so inclined, laugh at the errors of our predecessors, and ridicule the guesses in which they indulged as to the etiology of the disease in question. These guesses, we may say, referred the origin of this many-headed hydatid to breeding in-and-in, to malformation in the head, to leaving the tails of the lambs undocked, to the infliction of violent blows on the head, to the penetration of the skull by boring flies. Certain approximations, were, it is true, made towards the discovery of certain of the antecedents which are usually, but by no means always or essentially, connected with the real cause of the complaints; and among them we may mention the suggestions which certain accurate observers made as to damp weather, as to the youth of the subject, and as to its having been hunted by dogs, being concerned with the production of the “staggers.” Still, it was by experiment alone that the matter was finally settled; and the moral which we draw from this history is embodied in the advice which we will herewith give to certain sanitarians, to the effect that they would do well to compare their “feelings after” truth in the branches of hygiene which are still tentative with those which have at last resulted in triumph in the entozoological department of public medicine. Experiment is not always available in hygiene, nor, indeed, in physiology; but observation, at all events, may be accurate, and if it is, so will furnish us with what theologians call “regulative,” if it cannot furnish us with “absolute” truth.

It is half amusing, half alarming to read, at p. 59, that an “ill-designing person” liberated a dog which Dr. Cobbold had fed with the too common human hydatid, and from which he had

fondly hoped to obtain the diminutive *Tænia echinococcus*. Whether the "ill-designing person" intended mischief against Dr. Cobbold, or kindness to the "small black dog," and so mischief as against his own species, does not appear; but it does appear that by his interference Dr. Cobbold's experiment was interrupted, and Professor Nettleship's given a precedence, which has (see 'Proceed. Royal Society,' lxxxvi, 1866) enabled him to claim to be the first person in England who succeeded in rearing the "strobile" of the Echinococcus. We trust that he will be also the last who will attempt this feat within our four seas, and that both he and the "ill-designing person" will henceforward direct their activities into the channels which are less directly full of risk for themselves and for us. For the modes of dealing with the dangerous tumors which such experiments, and such disturbances of such experiments may both alike and equally give rise to, we have pleasure in referring to a paper by Dr. Church, of St. Bartholomew's Hospital, which, as having been read as a dissertation for a degree, may not have been published as well as printed, and may so have escaped from an enrolment in Dr. Cobbold's "Bibliography."

With Dr. Cobbold's comments on the grouse disease, we close this notice of his two volumes:

"It is not easy for people unacquainted with the phenomena of parasitism, and with the mode of distribution of the entozoa, to shake off the very prevalent notion that tapeworms are generally only in diseased or unhealthy animals. This old and erroneous idea has been handed down from age to age, and will probably prevail amongst us for many years to come. It is not my present purpose to offer any opinion respecting the actual cause of the grouse disease; but I am prepared, nevertheless, to show that it has no necessary connection either with the presence or absence of entozoa. It is one thing to admit the occasional destruction of game-birds from the prevalence of tapeworm, and quite another to suppose that their presence could give rise to the singular morbid appearances which were found in the diseased grouse of the year 1867. Amongst all the various birds and mammals from which I have removed entozoa post-mortem I never remember to have found the alimentary canal inflamed, and certainly there was no such disorganization of the liver and lungs as obtained in the case of birds dying of "the grouse disorder." No amount of tapeworms could ever give rise to gangrene and pyæmia of these organs; and even in cases where the parasites are sufficiently numerous to prove fatal to the "bearer," death never supervenes in the manner shown to have taken place in the grouse affection. As a rule, tapeworms and other adult forms of entozoa in animals are liable to produce emaciation, gradually destroying life when present in any considerable numbers; but in the "new disease" the birds were frequently quite plump at the time of their death, and the numbers of entozoa present were never very remarkable."—P. 66.

III.—Recent Works on Cholera.¹

THE reports of the Dublin hospitals on the cholera, collected and published in 1866, gave rise to an important discussion upon the nature and treatment of that disease generally, before the Medical Association of the College of Physicians of Ireland, during three meetings held by that association at Dublin, under the presidency of Dr. Stokes, in 1867.

We will endeavour to present our readers with a summary of the proceedings.

In the Meath Hospital, 130 cases were treated with an average mortality of 51·5 per cent. It was 54·5 in September and October, when the wards, improvised for the emergency, were overerowed and went down to 42·8 per cent., when the newly erected and airy sheds were opened in December. More than twenty cases died within five hours, and some within a quarter of an hour after admission. The aged and the dissi-

¹ 1. *Reports of the Dublin Hospitals on the Cholera of 1866.* ('Dublin Quarterly Journal of Medical Science and Medical Press and Circular.')

2. *Le Choléra, Étiologie et Prophylaxie, Origine, Endémicité, &c., &c. Exposé des Travaux de la Conférence Sanitaire Internationale de Constantinople mis en ordre et précédée d'une Introduction.* Par A. FAUVEL, Délégué du gouvernement Français à la Conférence, &c., &c. Paris, 1868.

3. *Address on Medicine delivered before the Annual Meeting of the British Medical Association at Leeds.* By Sir WILLIAM JENNER, Bart., M.D., F.R.S., Physician to the Queen, Physician to University College Hospital, &c. (Lancet, July 31, 1869.)

4. *Archives Générales de Médecine*, November, 1868, and January, 1869.

5. *Cholera in the East, from the Commencement of European connection with it.* By JOHN MACPHERSON, M.D., Inspector-General of Hospitals H.M. Bengal Army, &c. London, 1869.

6. *Étude sur le Choléra, &c.* Par le Docteur NICAISE, &c., &c. Paris, 1868.

7. *Pathologisch-Anatomische Studien über das Wesen des Cholera-Processes.* Von Dr. JULIUS MAR. KLOB, Professor der Pathologischen Anatomie an der Wiener Hochschule, Prosector des Krankenhauses Rudolfs-Stiftung, &c., &c. Leipzig, 1867. (*Pathological and Anatomical Researches on the Nature of the Cholera Process.* By Prof. Dr. JULIUS M. KLOB. Leipsic, 1867.)

8. *The Lancet*, 1869, p. 167.

9. *The Cholera; its Origin, Idiosyncrasy, and Treatment.* By FERDINAND E. JENCKEN, M.D., M.R.C.P. Lond. London, 1867.

10. *Cholera; its Symptoms, Clinical History, &c.* By S. GOODEVE CHUCKERBUTTY, M.D. (Lond.), Surgeon, Bengal Army, &c., &c. Calcutta, 1867.

11. *Report on the Cholera Epidemic of 1866, as treated in the Mater Misericordiae Hospital, Dublin, &c.* By Drs. HAYDEN and CRUISE. Dublin, 1867.

12. *Quarantine and Cholera.* By A. J. PAYNE, M.D., Bengal Medical Service. Calcutta, 1869.

13. *Della Natura del Cholera Asiatico, &c.* Memoria del Dott. FILIPPO PACINI. Firenze, 1866. (*A Treatise upon Asiatic Cholera, &c.* By Dr. PHILIP PACINI. Florence, 1866.)

14. *Report to the International Sanitary Conference, of a Commission from that Body, on the Origin, Endemicity, Transmissibility, and Propagation of Asiatic Cholera.* Translated by SAMUEL L. ABBOT, M.D., &c. Boston, 1867.

pated seldom recovered. Of 116 cases treated by Dr. Hudson, five were in the 'premonitory stage;' 108 in collapse or approaching it, and three in consecutive fever. Of the fourteen cases treated by Dr. Stokes, three were in the premonitory stage, nine in collapse or approaching it, and two in consecutive fever.

Treatment.—"In premonitory diarrhœa and early stage, castor oil was given in two cases, but with discouraging results;" acetate of lead with small quantities of opium, dilute sulphuric acid, chalk mixture, with kino and catechu "often succeeded." For vomiting, freshly prepared charcoal, and bisulphite of soda were given, but creasote and acetic acid were considered most useful. In cramps of the stomach, a solution of camphor and chloroform, with capsicum, were used. In severe cramps chloroform inhalation was useful, hot stupes of mustard and turpentine, frictions to the surface, followed by flannel bandages. Brandy and ice were also used internally.

In collapse, the patient was enveloped in a blanket wrung out of hot water and mustard and sprinkled with turpentine, over which dry blankets were placed; this was found "most efficacious," raising the temperature and restoring the pulse. Sir D. Corrigan's button-shaped cautery, heated with boiling water and applied over the vagus and on either side of the spine was useful at least for a time. Small quantities of calomel with bismuth, &c., were given internally. Cannabis indica was apparently useful, and brandy was freely given. Calabar bean was given in some cases, but the result did not justify its continuance. Saline injection into the veins was used twice, but the cases died after apparent and almost instantaneous temporary relief. In the "Stage of suppression of urine and consecutive fever," cupping over the loins to three or four ounces, hot bran poultices to the feet and diuretics, with calomel and bismuth, with but little stimulants, constituted the treatment. If the stools were bilious and copious, the calomel was either stopped or given in small doses at long intervals. If there was no contra-indication, besides a moderate amount of stimulants, chicken broth or weak beef-tea, bread and milk, or tea and ice were given. If uræmia with head symptoms continued, shaving the head, blistering the nape of the neck, and leeches to the mastoid processes, with calomel and rhubarb if required, were resorted to.

But two cases of cholera arose in the hospital, at a time when there were no cholera cases in the establishment,¹ one a case of a convalescent from typhoid, which died in nine hours; the other in the thirteenth day of typhus, with high

¹ The cholera wards were detached from the Hospital.—REVIEWER.

nervous symptoms, and died in twelve hours. A night nurse and a laundress attached to the cholera wards took the disease; the former recovered and the latter died. A woman six months pregnant came in with cholera and made a good recovery, and left the hospital without any sign of abortion.

In the House of Industry Hospital (Hardwicke) 277 cases were treated in the epidemic last year. Of these, 193 were "true cholera," "68 diarrhœa," and "16 vomiting and cramps." Of the first, 122 died, one of the diarrhœa, and none of vomiting and cramp cases. Females exhibited a heavier mortality at all ages than males except between the ages of ten and twenty. Ninety cases of cholera died within twenty-four hours after admission. The per-centage mortality of true cholera cases was 66·3, that of males being 60, and of females 70·8. It varied from 69·84 (the highest) in October to 55·5 per cent. (the lowest) in December.

No special treatment was exclusively adopted, each case being treated according to the chief symptoms. In "about thirty-three cases," astringents were chiefly used, with fifteen deaths to eighteen recoveries.

Of cases treated with calomel in small doses, thirty-one died to sixteen recoveries. In secondary fever, marked by suppression or diminished secretion of urine, nitre, calomel, dry cupping to the loins, the prolonged use of the warm bath, and diuretics were found "of essential service."

General summary of post-mortem appearances.—Brain, spinal cord, solar plexus and semilunar ganglia, healthy. Blood thick and tarry. Heart, right chambers loaded; pulmonary artery contained coagulum. Lungs, occasional congestion; no constant condition. Stomach, a washed appearance; intestines ditto, and of a pale colour; sago grain appearance of minute glands. Liver and spleen, no special appearances. Kidneys, rather bloodless; and, in cases of prolonged life with uræmia, some congestion with infarction of tubules.

Of 428 fever cases in the Hardwicke Hospital during the epidemic, five took cholera and died of it. Of sixty persons, physicians and pupils (who visited the hospital), and the resident staff nurses, &c., but two persons are recorded as having taken cholera, one of these, a pupil recovered, and the other, a laundrymaid, died. Five cases of cerebro-spinal arachnitis occurred in the sequelæ of cholera. True cholera typhoid was not seen as a sequel of cholera.

Mountjoy Male Government Prison.—Of 288 prisoners, but five were sick in hospital on 22nd December, 1866. On 23rd, five cases of malignant cholera occurred, of which three died before many hours. On 26th two, and on 27th two more cases

occurred; of the latter, one died. The whole number was nine, of which four died. The day after the first appearance of cholera, sixty-four cases of diarrhœa were treated. The outbreak commenced and ended within a week. Of the four fatal cases but one had premonitory diarrhœa (forty-eight hours), and one had collapse from the first. The medical officer, Dr. R. M'Donnell could not trace the origin of this local attack.

Sir P. Dun's Hospital. — 2nd August, the first case of cholera was admitted; 180 cases of cholera were treated with a per-centage mortality of 47·29, of recovery 52·71. Of 148 of these, 50 per cent. collapsed on admission; 4·06 had not purging, 12·17 had not vomiting, and 43·25 per cent. had not cramps during their illness. Of the six who had no purging 50 per cent. died; all of these save one were females; "one suffered only from cramps," dying in six hours.

A boy, æt. 6, who had only vomiting, gradually showed signs of tubercular meningitis with effusion, of which he died 408 hours after admission, the autopsy verifying the diagnosis.

Two remarkable instances of mental shock occurred in connection with the disease, one being fatal, the other recovering. In two cases the commencement of the disease dated from the taking of a dose of castor-oil. Errors of diet appeared in a vast number of instances to be the predisposing causes.

Cramps were most common in the lower extremities, less so in the upper, and least common in the muscles of the abdomen. Suppression of urine, varying in degree, was common. In such cases coma frequently preceded death, without any other sign of uræmic poisoning. The suppression lasted three days in a few cases. One of these persons recovered; another, a man, aged 39, with suppression continuing at the end of the third day, had four ounces of blood taken from the arm, and in half an hour he secreted and passed some urine. This reminds us of a nearly similar case, mentioned already in the 'Review.'¹ In another case of a like nature the same measure failed. Dry cupping over the loins appeared useful in another case with suppression. The cadaveric movements were in several instances extremely noteworthy. The blood was found of a thick, black, and tarry consistence.

The general line of treatment was to arrest the diarrhœa and vomiting as soon as possible. Acetate of lead and opium were used, and if necessary acetate of lead (up to 15 grains) with half a drachm of the acetous tincture of opium was given by injection. Dr. Kennedy gave mustard emetics to all his cases on admission. If vomiting troubled, hydrocyanic acid, even up

¹ July, 1866, p. 159.

to 6 minims (dilute), with or without Schacht's solution of bismuth, were given; acetate of lead, spearmint-water, sinapisms, and mustard emetics, were also used for the same symptom. For the cramps chloroform inhalations were in some cases used. In and previously to collapse, internal and external stimulation was freely tried. Hot stupes of turpentine to the hypogastric region were applied in suppression of urine, though secreted urine was sometimes retained, and the catheter required. Permanganate of potash and calomel were not thought much of. One fifth of all cases died within twelve hours after admission.

Of the 180 cases the per centage was 52·22 females and 47·78 males. Of the former 51·76 per cent. and of the latter 48·24 per cent. died. The heaviest mortality was 54·54 per cent. in August, and the lightest in October, when it was 45·45, but it increased again to 48·14 in November. Each case had 2500 cubic feet of air, "hence the good health of the attendants, and in a great measure the recoveries of those who suffered from the epidemic." One case, included above, was found dead on arrival at the hospital. The mortality between ten and nineteen was but 57·03, under nine years of age 41·38, and between twenty and twenty-nine 41·93 per cent. It was heaviest, 66·66, between sixty and sixty-nine. The only case over seventy years old recovered.

Mater Misericordiae Hospital.—197 cases were treated in this hospital with a mortality of 106. Of *choleraic diarrhœa* fifty-four were admitted and two died. Of collapse cases eighty-five died and thirty-nine recovered. "The numbers did not include nineteen cases which were moribund when admitted."

"Numbers of cases in which attack was due to any assignable cause not including infection, ninety-two;" neglected diarrhœa, sixty-five; intemperance, eighteen; "foul water," two; cockles, sour porter, cold cabbage, fish, pork, badly-cooked cabbage, and dentition, of each, one. The mortality between ten and twenty years of age was 54·0; between twenty and thirty it was 33·3; between sixty and seventy it was heaviest of all, viz., 85·7; between seventy and eighty it was but 42·8; and over eighty years it was but 50·0 per cent. These figures, it will be observed, varied from those of some of the other hospitals.

In the stage of choleraic diarrhœa, dilute sulphuric acid and opium and creosote water (mij to a pint of water) *ad libitum* was much given. In the stage of collapse, calomel (gr. x at once and gr. ij each hour to adults; smaller doses to children) was a good deal given, with other treatment. Nitrate of amyl and oxygen were given by inhalation in some cases, with but slight temporary elevation of temperature from the use of the former.

Post-mortem appearances in collapse:—Rigidity of very short duration, less lividity, and sometimes higher temperature than before death. Cerebral veins and sinuses gorged with dark liquid blood; some clear serum in ventricles and subarachnoid spaces; puncta cruenta large and numerous; spinal cord and membranes normal, as were also the solar plexus and semilunar ganglia. Lungs congested, but still crepitant; right cavities of the heart filled with dark uncoagulated blood; left auricle and ventricle contracted and empty—the latter much reduced in size, and its walls thickened. Liver, spleen, and kidneys normal; peritoneal investment of viscera white and polished; enlarged mesenteric glands; much gruel-like fluid in stomach; mucous lining of œsophagus prolapsed into stomach; intestines nearly full of thin, boiled, starch-like fluid with white flocculi. The glands were generally distended. Towards the termination of the ileum Peyer's patches were in a state of erosion. Larynx.—Vocal cords thickened and ventricles reduced in size, but without vascularity. Cholera did not seem to spread in the hospital. Disinfection and ventilation were well attended to.

At the second meeting of the Medical Society of the College of Physicians, Dr. W. D. Moore read an abstract of a paper, by Dr. Snellen, from the Dutch 'Archives of Medicine,' on a localised epidemic of cholera, in Utrecht, of great severity. In that city of 60,000 people 2,400 cases of cholera occurred between 6th April (1866) and the middle of September. In Hospital Lane, where the localised epidemic occurred, the disease lasted but twenty-five days, attacking 17 per cent. and killing 10 per cent. of its inhabitants. The numbers of men and women attacked were remarkably similar, being 16 per cent. of the former and 16·5 per cent. of the latter. Dr. Snellen describes the sanitary state of Hospital Lane, which, though far from perfect, was by no means worse than unaffected places near at hand, and asks, "Whence in this case the local violence of the epidemic, so extremely limited in time and space, running its course in twenty-five days, confining itself in its immediate vicinity to one street?" No special causes were discoverable in Hospital Lane, nor any appreciable difference in the observed phenomena there, and those attending the commencement of the disease in other parts of the city.

The author tried experiments on animals, but did not succeed in giving the disease to them. Though "all are agreed that the disease arises epidemically in no place where it cannot be conveyed by the sick, or by what was in contact with the sick," the difficulty appears to be to explain how it is transmitted. Dr. Snellen considers that "*it is exclusively the digestive organs by*

which the infecting matter enters the body." He advocates, as the best safeguards against cholera, good water and food, cleanliness, good dwellings, and the investigation of endemic diseases.

Dr. Darby, of Bray, pointed out the divergence of opinion existing as to what constituted cholera, and hence the varying reports of death-rates in the disease. Dr. Darby regarded all diarrhœa cases with cramps and vomiting as cholera, and fairly argued that we do not regard the malignant cases of scarlatina and of typhus as being alone representatives of those diseases, to the exclusion of mild cases. He illustrated his opinions by reference to sixty-one cases treated by him in the Rathdown Hospital, from 28th September to 28th December.

We think this subject deserving of attention, as we have seen last year, as well as on previous occasions, much mischief arise from the spread of the disease from cases regarded as only diarrhœa; and we would impress on our professional brethren the propriety of keeping the authorities alive to the necessity of separating such cases from the healthy, and of providing prompt medical attendance for them.

Dr. MacEwing wished for "a fixed uniform basis of tabulation" of the facts of the disease; a proposition that was not recognised as of much value by Dr. Lyons, who disbelieved the spread of cholera from the sick to the healthy and the utility of quarantine. Dr. Hayden regarded the disease as conveyed only by man, his dress, furniture, &c. Dr. William Moore showed the necessity of house-to-house visitation, sufficient hospital accommodation, and of more active and decided sanitary measures than had been adopted in the recent epidemic. Dr. Henry Kennedy avowed himself a non-contagionist. He spoke of epidemics always travelling from east to west, or from south-west to north-west. As to treatment, he praised mustard emetics in cholera, regarded puerile respiration as a sure precursor of death, and found the second heart-sound lost; both these phenomena he attributed to the state of the blood in cholera. "It had ceased to circulate, or nearly so; and, as long since shown by Sir D. Corrigan and others, the valvular sound of the organ—that is, the second—must necessarily then be lost."

Dr. Laing, staff surgeon-major, spoke of the apparent connection between high temperature with moisture of the atmosphere and cholera in his experience in India. He advocated regarding every case of diarrhœa during an epidemic of cholera as a case of cholera. He recommended moving the inhabitants out of an infected locality. He treated the diarrhœa stage by opium and astringents. Dr. Hansty and Dr. Murrough, from

what they had seen in India, regarded cholera as not contagious, and, from their Indian experience, advocated checking the diarrhoea and vomiting, and were opposed to purgatives and emetics.

On the third night of meeting Sir D. Corrigan described his cholera map of Ireland, and went on to show that the more secluded towns of the west and south-west of Ireland were those that chiefly suffered from cholera. Alluding to the numbers of those who did not take cholera in Dublin in 1866, he stated that "the contagious nature of cholera had never been demonstrated," though he looked on it "as a disease in which contagion might become an element."

Mr. Comyn said that, from his experience in Malta, he thought the portability of cholera was more evident in southern Europe and at home than in India. He mentioned a case of the reappearance of cholera in a house in Malta which seemed to show the latency in the disease for the period of a year. Of twenty-six medical officers serving at Gibraltar, many of them having long Indian experience, he did not believe that one was a non-contagionist. He showed by the general exemption of Malta last year, the value of efficient surveillance or quarantine over shipping. Dr. Head described the remarkable connection between the appearance of cholera in a secluded village in Downshire and the arrival of a fishing boat from the county Dublin, with cholera on board, observes, "That it was difficult to explain the outbreak of cholera in that remote and isolated locality, except on the theory of contagion." Dr. Cruise and Dr. White regarded the disease as contagious; and Dr. Mapother advocated the views of Pettenkofer, dwelling also upon the returns of the Registrar-General and Dr. Burke, which showed that in almost every one of the thirty districts attacked in Ireland the importation of the disease was traceable. He also spoke of the influence exercised by old water-courses upon the spread of the disease.

Rev. Professor Haughton reviewed the different facts mentioned in favour of and against the contagion of cholera. He spoke of the value of Sir D. Corrigan's map of the cholera of 1849, and illustrated his own observations and inquiries by a map of the recent epidemic. He likewise remarked upon the great facilities afforded by modern rapidity of travel for the quick and extended dissemination of the disease. He advocated greater precision in future in the endeavour to trace the particular channel by which the disease entered the body, and in finding out the condition of ill-health favouring susceptibility to it.

Dr. C. F. Moore, as a city district medical officer, in whose

district 190 cases of cholera and much over 1000 of diarrhœa were under treatment, showed that the earlier cases had generally been in affected districts before taking ill, and that over-crowded and otherwise unwholesome cellars and other tenements appeared subsequently as foci, whence the disease extended in various directions. This state of things resembled what he had witnessed in 1854 in Ireland, and in previous epidemics in England, Egypt, Malta, &c. In conclusion he quoted recent writers on the disease in India demonstrating the tendency of the disease there to spread from the sick to the healthy.

The president, recapitulating the proceedings, said, "anatomy told them what cholera was not, not what it was; its information was negative, and there was one reason for this besides others, namely, the short course the disease runs, so that there is not even time for the formation of those secondary organic affections which so commonly arise in essential diseases." He reviewed the usual post-mortem appearances, alluding to some points of analogy between cholera and sun-stroke, and the form of disease called by some "purpura maligna," and by others "black death," where the large petechial or ecchymosed spots, almost black at the time of death, became red on exposure to the air. The right chamber of the heart was found full of blood and a clot in the pulmonary artery. Puerile respiration (mentioned by Dr. Kennedy) he regarded as a very important phenomenon resembling other cases of weakness of the heart. The brain and spinal cord had been generally found healthy; the washed appearance of the intestines, and in some the bursting of Peyer's and Brunner's glands, and the cadaveric twitchings of the limbs were noticed by Dr. Stokes. As to treatment, no specific can be sought for in cholera; we must guide the case through the dangers of debility on the one hand, and of secondary disease on the other. It was an error to have placed in the reports of some of the hospitals, a column for special treatment, for, as Sir D. Corrigan had said, Irish medicine was characterized by eclectism. As a palliative he would mention the value of chloroform inhalation, not to induce insensibility, but as an anæsthetic. He also shortly enumerated the other chief points of treatment already mentioned above. The question of the non-contagiousness of cholera had some supporters, and others thought it less contagious in India than at home, which might be the case, as there were many points of differences as to climate, &c. Dr. Mackay considered the European constitution less liable to contagion in hot climates. Dr. Lyons had argued against the contagion of cholera, as few attendants at the Hardwicke Hospital had taken cholera. But we must remember the terri-

ble mortality among medical men in the epidemic of 1849: five medical men, for instance, died of it in Sligo, &c.

M. Fauvel's work furnishes a very elaborate compendium of the proceedings of the International Sanitary Conference held at Constantinople in 1866, and of which a notice was published in the July, 1868, number of this Review. The labours of the Conference have attracted much attention, as seen in the many works which have treated either solely or in part of them. At the risk of some repetition we propose to notice some of the principal conclusions arrived at.

The Conference divided itself into sections, each section taking separate questions. By this division of labour the origin and genesis of cholera, its importation and transmissibility, the agents of its transmission, the question of immunity in reference to countries, to localities, and to individuals; in each case, though surrounded by the disease, the influence exercised by all gatherings of human beings, whether on land or at sea, the hygienic conditions in connection with the causes of cholera epidemics were considered in detail.

These several points, as well as the summing up of all the facts obtained, the deduction "from them, so far as possible, of a theory of the properties of the generative principle of cholera, with special reference to prophylaxis," and a general view of the course and propagation of the disease in 1865, were treated in detail. A commission likewise was formed, consisting of the whole of the physicians, twenty-one in number, and three diplomatists, to discuss and condense the reports of the sub-commissions. M. Fauvel was appointed reporter general.

The reports having been thus arranged and condensed were once more reviewed and adopted by the full commission before being finally submitted to the Conference.

The Conference was content rather to place in strong relief the fundamental character that distinguished Asiatic cholera from ordinary cholera, than to insist upon all the characteristics of the two disorders, but in reality there is no identity in their nature; there are certain symptoms common to both and nothing more.

Indian cholera differs, not only in its great fatality, but also in the very grave and often fatal morbid state which remains if the disease does not kill in what may be called the cholera or early stage, and that even in cases proportionally mild.

Great difficulty, no doubt even impossibility, would be met with in the attempt to distinguish, at a first visit, between Asiatic and ordinary cholera, especially when the former is epidemic. But mutual resemblance and insufficiency of the

means of diagnosis does not establish the proof of identity between the two diseases.

Finally, the Conference adopted this fundamental law on the subject :—*‘Jamais le choléra né en Europe n’a pris le caractère envahissant, ou bien, en d’autres termes, jamais on n’a vu une épidémie de choléra développée PRIMITIVEMENT sur un point quelconque de l’Europe devenir l’origine, le foyer propagateur d’une épidémie envahissante.’*

In other words, ordinary cholera, as met with in Europe, never assumes the invading character ; it never becomes a centre from which the disease spreads to other countries.

The Conference states, what was long since published to the world by that highly gifted physician Graves, that cholera never travelled more quickly than (the means of transport then) ships. In our day steam has arrived at a state of perfection not attained in his time. The air alone does not suffice as a means of propagation of the disease. Human intercourse is the essential in its diffusion. Two agents are necessary for the carriage and diffusion of cholera, and they are the arrival of a case of the disease and favouring circumstances. A person with the disorder upon him need not necessarily be suffering from more than choleraic diarrhœa.

Clothes soiled with cholera dejections also suffice for its propagation, especially if they have been kept without free exposure to the air. This power seems retained by soiled clothes, if shut up for a very long time.

Merchandize, animals, and the bodies of persons dead of cholera, should be treated as capable of carrying the contagion, though we have not certain evidence of the propagation of cholera by these means.

The French authorities, as noted by the ‘Archives Générale,’ state that the last epidemic, that of 1865, has demonstrated that drinking waters containing cholera “dejections” become most energetic agents in its dissemination. This fact, long ago insisted on by Snow and others, is now placed, the Conference says, beyond dispute ; and the necessity cannot be too constantly enforced on the public of having supply cisterns, not only properly cleansed when first constructed, but also maintained free from all pollution and taint.

There is great difficulty in determining the duration of the period of incubation of cholera, the disease not appearing in some cases, as on board ship, for twenty days, or even more, after leaving port. In such instances, however, it would appear doubtful whether it was really absent during that time. The Conference concluded that if an individual escape all cause of contamination for eight days, and presents no symptom of the

disease, he may without danger be admitted into a county free from cholera.

Ships, railways, and, lastly, caravans across the desert, are, in the order mentioned, stated by the Conference to be the most likely and usual means of transport of the disease.

It was also held to be proved that if a caravan journey across the desert exceeded twenty days, that the travellers became free from cholera in that time, though suffering from the disease at the commencement of the journey.

Macpherson, Griesinger, and others, doubt the correctness of this statement. Our own knowledge of the habits of pilgrims, of their clothing, &c., would lead us to regard caravans as the very vehicle for the communication of the disease, notwithstanding the pure desert air.

On the influence of crowding Dr. Fauvel reports thus—

“Every collection of human beings in the midst of which cholera has been introduced presents a favorable condition for the rapid extension of the disease, and, if this collection exists under bad hygienic conditions, it will account for the violence of the epidemic among them.

“That in such a case the rapidity of extension of the disease is proportionate to the concentration of the agglomerated mass, while the violence of the epidemic is, other things being equal, so much the more decided as the individuals composing the crowd have been little exposed to the choleraic influence, or not at all; that is to say, in other words, that individuals who have already been exposed to the influence of a focus of cholera enjoy a sort of relative and temporary immunity, which counterbalances the bad effects of crowding.

“That, finally, in a dense crowd, the more rapid the extension the more prompt also is the cessation of the epidemic, at least if new arrivals of unaffected persons do not furnish new aliment for the disease, and keep it alive.”

In now dismissing this report of the Conference, it becomes necessary to observe that the progress of science, as well as of the present state of rapid intercommunication of nations, are both opposed, for the most part, to the conclusions arrived at and the remedies proposed by the Conference, and that it is as impossible for any such assembly to cause the medical mind to regress, if we may use the word, as it would be to stop international communication as at present existing. This seems to us little to be regretted; for how much better is it to know that the promotion of sound health among nations is the surest preventive of fatal epidemics, than to go on with the antiquated notions that we can stop a plague by shutting up the infected and suspected together in a lazaretto.

On the other hand, if measures of sanitary improvement such as

are mentioned by Dr. Fauvel as having been carried out at Mecca recently, are adopted generally in the East, much good indeed will surely arise. Great Britain has done much at home and in India in this direction with the happiest results.

The recent address in medicine, delivered at the Leeds meeting of the British Medical Association by Sir William Jenner, bears so much upon the subject of the prevention of cholera, that we think it well to quote it here :

“ In illustration of the advances made in preventive medicine, I would adduce the fact that drinking water is one of the greatest agents for the spread of two of the most fatal and acute diseases of the present time, namely, cholera and typhoid fever.

“ Now, with reference to cholera, the special facts collected by Dr. Snow prove that one of the great agents in the diffusion of cholera is drinking water, that every virulent local outbreak in a limited district was clearly coexistent with the pollution of drinking water supplied to that district, and that persons living at a distance, if by accident they drank of the polluted water, suffered as certainly as if they dwelt in the district specially affected. The conclusion which follows from the facts collected by Dr. Snow is, that the conditions existing, be they atmospheric or otherwise, which determine an epidemic disposition to cholera, the presence of minute particles of cholera excreta in water supplied to a district for drinking purposes will be followed by an outbreak of cholera in that district. Careful investigations into the circumstances attending the local outbursts of cholera during the last epidemic have proved the truth of the conclusion. I will refer to two such special investigations in confirmation, viz. Mr. Radcliffe’s admirable researches into the relation between the water-supply and the spread of cholera in London, and to Dr. Bellot’s most conclusive observations on impure water as a cause of cholera in Holland. Dr. Snow’s investigations traced special individual cases of local outbreaks to one exciting cause. Mr. Radcliffe’s researches bear especially on the influence of the polluted water in determining excess of mortality in a large district of a great city. Dr. Bellot’s facts show that those towns and those parts of a town in Holland in which there was the greatest facility for the contamination of water-supply by cholera dejections were those which suffered by far the most severely. The spread of typhoid fever by the contamination of the drinking-water supply is, if possible, less disputable than is the spread of cholera by the same means.”

Sir W. Jenner proceeded to observe that cholera and fever

“ Occur like smallpox, scarlet fever, and measles, *as epidemics*, owing to causes of which we know little or nothing ; but when epidemic, unlike smallpox, scarlet fever, and measles, a local outbreak of cholera and of typhoid fever will be determined by the impurity of the drinking water. Had the water supplied to the east of London been as free from organic impurity as that supplied to the

west of London, the death-rate from cholera at the east would have been a little larger only than was the death-rate at the west of London. Had the drainage and water-supply of Winterton, Terling, and Guildford been what modern medicine has shown for health purposes they should have been, these places would not have suffered the terrible outbreaks of typhoid fever, of which the medical officer of the Privy Council gives such full details in the tenth volume of his inexpressibly valuable reports. The persons who died at these places from typhoid fever, and a large proportion of those who died at the east of London from cholera, were as certainly killed by the water they drank, and killed without need, as if the water supplied to them had been contaminated with arsenic. And I am sure we all agree with the most distinguished medical officer of the Privy Council, that 'the distribution of foulest water by the Guildford Board is as proper a case for judge and jury on action for damages, by any of the 500 people who had typhoid fever in that town, as any case in which a railway collision brings some score of passengers into harm; and the fact that these water-purveyors gave typhoid fever to their customers would be brought home to their consciences, and be suggested as a warning to other water-purveyors in a far more conclusive and effective manner, by such legal proceedings, than it can be by any departmental statistics and remonstrances.'

Dr. Macpherson has carefully collated several authorities "on the early seats of cholera in India and in the East with reference to the past and the present." The readers of the 'British and Foreign Quarterly Review' will remember the many important services rendered by Dr. Macpherson in connection with cholera; and it is well that one so well informed on the subject, and possessed of so much information on Indian matters, should have laboured to give a history of the disease, extending back for centuries beyond the usually given date of 1817, as such research becomes all the more important in relation to the inquiry held by the conference at Constantinople. Dr. Macpherson, it will be remembered, dissented from some of the conclusions drawn by other members of the conference; he discards the consideration of the statement given in Sanscrit, Greek, and Arab writings, confining his remarks to the period of European connection with the East.

The disease existed under very varying circumstances, in many parts of India offering the greatest difference in their geological and geographical characteristics, in valleys, on the seashore, on high table lands, by river sides, and on arid plains.

Dr. Macpherson contrasts statements quoted by him from the writings of several medical and other authors, from the time of Correa in the beginning of the sixteenth century down to the Bengal and Madras Reports in 1817, with the conclusions arrived at by the majority of the conference.

It will have been seen from our observations in an earlier part of this review, that the inference arrived at by the majority of the members of the conference is opposed to observations made by men of experience having no object to gain, and no theory to advance by the expression of their opinion and belief; we allude to the doctrine that caravans do not spread cholera if travelling or exposed to the desert air for twenty days.

Having quoted several authors on the endemicity of cholera in many parts of the East, without the bounds of India, during many seasons, and having shown the mode of its extension to have been by caravans in frequent instances, Macpherson observes—

“Whatever importance we may assign to the great outbreak of cholera in Bengal in 1817, cholera still prevails in its oldest seats, and on the side of India in closest and nearest communication with the Persian Gulf and the Red Sea. The common notion that cholera is the product of the low plains of India appears to be groundless, and the importance attached to the Gangetic valley is quite exaggerated. The French idea of uprooting cholera by draining the delta, and as one gentleman put it, by running about twenty canals through it, can scarcely be entertained by any sane mind. Supposing the delta of the Ganges to be improved, and cholera lessened, one source of the disease would be diminished; but could we imagine that this would have much influence on the other parts of India where it flourishes independently? The idea expressed in some of the latest French writings, that cholera is the result, not merely of English neglect of public works, but of the mental and physical decay, and of the cerebral degeneracy of the natives of India, consequent on English rule and *exploitation mercantile*, is absolutely too absurd for serious consideration.”

Orton long ago pointed to the danger arising from cholera excreta as foci for the spread of the disease. Macpherson, Simon, and many others dwell upon this important matter.

Macpherson points out the inutility of quarantine enforced in the Red Sea, unless also carried out all along the Arabian coast and that of the Persian Gulf, an impracticable measure.

Dr. Nicaise's essay contains his own experience of the cholera of 1865-6, as well as observations from other authors on the same subject.

He notes that the nature (*génie*) of the disease in each year was the same; that it was, in fact, the same epidemic, which was only suspended by the winter cold. We would here observe that this fact was overlooked by Sir Dominic Corrigan, as seen in an earlier part of this paper, when he argued that because the disease remained dormant in the west of Ireland during some months of winter, it was not connected with the

reapers who came from infected localities in England in the preceding autumn.

The disease was not quite latent in the winter, as it clung to some parts of the sea-coast, and during the succeeding summer 1866 again became general.

Allusion is made to the coexistence with the disease or its precedence by epizootic disease. Before the cholera appeared cattle and fowl were attacked in France; in the case of fowl, diarrhœa formed a prominent symptom of the disorder. This fact is mentioned by Dr. Nicaise in the hopes that the connection between the two circumstances may yet be discovered.

The much greater severity of hospital cases in comparison to cases generally met with in out-practice is also remarked, and yet is not uncommonly lost sight of by many, the reason being, however, obvious, namely, the general reluctance of the sick to enter an hospital until in confirmed cholera.

Professor Julius M. Klob mentions the uncertainty of classification of the simplest organic forms; he regards the so-called "Vibriones" and "Bacteriæ" as parasites.

Cholera evacuations, as well as the contents of the intestinal canal after death by cholera constantly contain epithelium (pavement and cylindrical) blood and mucus, also "*sporonea gracile*," crystals of triple phosphate, and "hæmin crystals" of Wedl. M. Klob has found the following as non-essential constituents—sarcinæ, fat crystals, crystallised urea and tyrosin.

A plate is given illustrating the several substances enumerated above, their more usual mode of combination, &c.

Mucous and pituitous masses generally constitute the principal part of the intestinal contents in cholera typhoid, and are then always of a biliary colour; rice-water evacuations and fresh fæces are always mixed with mucus. The author treats at length of the presence of many very minute dots finely dusted over the mucous masses. These dots are also observed to grow into small granules having a clear centre and an opaque contour.

"The smaller the aggregations (of granules) the more thickly do the granules lie together; the larger, the more clearly does their transparent medium appear."

Again, he remarks:

"It seems clearly to follow that the aggregations grow in size, and through division increase in number, that this division through various and probably external contingent conditions results in different modifications of the external form."

This author is inclined to assume that—

“The granules themselves through a peculiar organic mechanism form that gelatinous or mucous intergranular substance, and finally by division of the mass give origin to such bulbous lumps. Viewing the question in this light, it may not be intestinal mucus in which these elements are found, but a substance proper to them, and bearing along with its development a most intimate connection in property with jelly.”

He cites Colin's researches in support of his views, and observes—

“That the spores, through a peculiar process, form a gelatinous mass which must not be confounded with the intestinal mucus, and which in many cases composes at least the principal part of those mucous materials which are found in the intestines.”

M. J. Lemaire attributes cholera, in common with typhus, plague, yellow fever, dysentery, intermittent fever and hospital gangrene to putrefying matter; the buboes, anthrax, moist gangrene, petechiæ, gastro-intestinal, and other symptoms met with in the disorders just named, he also refers to this one common cause.¹

Rev. M. J. Berkeley,² whose abilities and competence on the subject entitles his observations to the highest consideration, doubts the soundness of the views advanced by Hallier on the so-called cholera fungus. We therefore look forward with much interest to the results of the inquiry now undertaken by the specially trained professional men to whom the British Government have intrusted the investigation of the subject in India.

Dr. Jencken's views may *perhaps* be gathered from necessarily short extracts:

“Visitations of sickness, therefore, come to us of a sudden and without warning, not by the mouth or by the nostrils, not breathed into the lungs or swallowed into the stomach, as some are wont to describe their ingress, but simply by the act of pervasion: the whole system being at once imbued with a destructive ethereal element, which, being too refined to touch the gross organic matter, immediately associates with the plastic ethereal envelope of the organic power, whose equilibrium it deranges, the consequences to the organism varying in accordance with the individual nature of the disease.”

“In my opinion, the collapse is chiefly due to a sudden stoppage of innervation, by the irresistible assault on the system of a deadly element; the nervous system, as in all deeply penetrative operations, being affected in its ethereal investiture, and thus disabled from

¹ ‘Archives Générales de Médecine,’ Novembre, 1868.

² ‘Lancet,’ p. 167, 1869.

presiding any longer over the vital functions of the body," &c., a very ethereal and intangible hypothesis.

As to remedies he observes :

"What is urgently required is a compound from the nitrogenous group, with a direct bearing upon the seat of injury, and possessing sufficient strength and stability to withstand the repeated inroads of the raging toxic element."

Dr. Chuckerbutty points to the great importance of cholera as a disease, causing so great a mortality as it does especially in India. He considers the use of astringents and opiates in the first stage as of the utmost importance, and regards opium as never tending to produce head symptoms unless uræmia be also present, provided the opium be not given in "poisonous doses."

In a preceding sentence he mentions one or two grains of solid or of powdered opium, or twenty to thirty minims of laudanum, as a full dose for an adult generally used.

From this author's large experience of the disease at Calcutta he is induced to recommend opium thus given, as frequently cutting short the malady in the first stage ; he, however, enjoins consideration of the susceptibility of different ages and constitutions, and the caution of discontinuing in collapse and reaction.

We have been led, from what we have ourselves seen, to know that coma may be induced even in the first stage by such doses as above indicated, if often repeated. Of all astringents chalk mixture is recommended as about the most generally advisable. It may, of course, be combined with opium or morphia ; caution is required in using stimulants for fear of narcotic action supervening from their prolonged use.

Where great irritability of stomach exists he has found cold or iced drinks and bland liquid food, with or without a little salt or lime-juice, useful. In some cases, reaction may be favoured by hot and spiced drinks. Milk and broth are apt to disagree.

When symptoms are "so threatening that something in the way of medicine must be attempted," he has seen a single large dose of calomel, 3ss, for instance, act as a charm, restoring warmth, &c., but he has never seen good, but only harm, induced by a repetition of the dose. We have already quoted Dr. Goodeve's experience and our own against calomel in such a dose, and we would here say that we have succeeded in inducing natural warmth, return of the secretion of bile, &c., by one-grain doses of calomel or grey powder, each hour, or each alternate hour, and we would not continue even these small doses for any great length of time, if such good results did not soon arise.

Friction of the whole body with ginger powder is another recommendation made by Dr. Chuckerbutty. If uræmic poisoning set in, dry cupping or taking blood by cupping, or by leeches, from the loins, and diuretics are advised, or calomel or castor oil; but though these measures sometimes succeed, failure very often arises. If uræmic convulsions, stupor, and coma occur, the head must be shaved and a blister applied to the vertex or to the nape of the neck.

The importance of considering the cases of primary cholera, and of those arising in the course of other diseases, or intercurrent cases, is dwelt upon by the same writer, but this we noticed in a previous number of the Review when treating of the works of other authors in 1866.

Of 63 deaths from cholera, 52 were considered primary cases and 11 intercurrent; of the former—3 were ascribed to exhaustion, 26 to collapse, and 23 to reaction; of the latter, 2 to exhaustion, 5 to collapse, and 4 to reaction.

We do not think many pathologists will agree with Dr. Chuckerbutty in the following:

“Cholera is not due to any poison introduced from without; it arises from a peculiar condition of the atmosphere, which causes a catarrh of the alimentary tract, which, according to circumstances, may terminate in the first stage, or pass on to the second, and then terminate, or be followed by the third.”

Speaking of the desire that those in cholera have for cold drinks and ice, the following is stated:

“Cold is more grateful than heat. Herein lies the great distinction between the collapse of cholera and the cold stage of intermittent fever. In the latter affection the patient feels very cold, shivers, and wishes to be covered with four or five blankets; in the former he complains of heat, throws off all clothes, and courts cold drinks to allay the sensation of heat,” &c.

During four years, commencing 1850, the mortality of cholera cases was, in Dr. Chuckerbutty's hospital practice, 41·5 per cent., and in “four years from 1860,” 47·9 per cent.; a mortality, he observes, “still considerably lower than in the practice of many others.” The rate of mortality he has found as high as 75 per cent. at the commencement, and sinking as low as 25 per cent. towards the end of an epidemic.

The record of post-mortem appearances of 63 cases deserve more detailed notice than it is in our power to give:

“The alimentary canal seems to be the only organ invariably affected, and next in frequency the kidneys. The third in fre-

quency and importance are affections of the lungs. These were congested in fourteen out of twenty-six cases," &c.

"The presence of congestion in more than one half of the cases is a fatal blow to the theory of Dr. George Johnson, who maintains that the lungs are always anæmic in the stage of collapse, because there is spasm of the pulmonary capillaries."

Another argument this author quotes against Dr. George Johnson's theory is taken from the state of the heart, the right cavities of which he has found full of blood in half the cases (of collapse), "and in about a quarter, likewise, the left cavities."

Prophylaxis of cholera is not quarantine, which Dr. Chucker-buty regards as "absurd," but sound hygienic measures.

Drs. Hayden and Cruise—whose report of cases at the Mater Misericordiæ Hospital we have already presented to our readers in abstract—published an extended paper in the 'Dublin Quarterly Journal,' in which several important cases of the spread of cholera *by contact* are given. These authors regard diarrhœa as the earliest manifestation of cholera, and they look on the disease itself as "strictly preventible by sanitary and hygienic measures, and in nearly all cases curable in the stages preceding actual collapse."

"In collapse we have had (add Drs. Hayden and Cruise) more success with calomel given in large doses than with any other medicinal agent. In several cases, besides those in which recovery took place, reaction set in under the calomel treatment, but death occurred in the consecutive fever."

We may here remind our readers that we have already pointed out in the Review a danger which we consider we have seen to arise through the calomel treatment, namely, its injurious action, probably as a chemical agent primarily, upon the patient when given to a large amount, and in this opinion our observations first made many years since in the East have been subsequently corroborated by the wholly independent experience of Dr. Goodeve.

Drs. Hayden and Cruise regard cholera as contagious, but less so than typhus, scarlatina, measles, and small-pox. They consider that a good state of health, and proper sanitary and dietetic precautions, afford a strong assurance of immunity from attack. They insist on *immediate* attention to derangement of stomach or laxity of bowels during a cholera epidemic.

They dwell upon the value of disinfection; we notice this to remind our readers of its signal failure in some places to which we have already in a previous number referred, and, we think,

we need hardly again dwell upon the importance of care as to contamination of water supply.

Dr. A. J. Payne, of the Bengal Medical Service, has ably investigated the question of quarantine in relation to cholera. The failure of quarantine in the first European epidemic—that of 1831-32—led to disbelief in the possibility of communication of cholera from man to man (this, no doubt, was generally an accepted doctrine; eminent men there were, of whom the name of Graves at once comes to our mind, who did not hold that view). The Conference of Constantinople has laboured to renew confidence in quarantine, which, if enforced “with the accuracy of a chemical experiment,” as Mr. Simon remarked, were that possible, should prove effectual in preventing the admission of the disease, provided personal contact were the sole means of its propagation.

But the impossibility of enforcing such a quarantine, and the amount of evidence showing that air, water, the soil, clothes, and other matters, are capable of spreading the disease, corroborated by the great amount of evidence that countries protected by the most rigid quarantine have, nevertheless, suffered far more than England, where quarantine is impossible, in consequence of the magnitude of her trade, &c.; but where sanitary measures, far more effectual than quarantine, have limited the extent of the disease, and where really effectual prevention of the use of polluted water, soil, &c., promises a far better result than any system of quarantine, however perfect.

In support of this position, we would refer our readers to the Report of the Sanitary Commissioners with the Government of India for 1867, which the ‘*Lancet*’ thus summarises :

“Taking the history of the Hurdwar outbreak as a whole, we may fairly regard it as affording very strong corroboration of the water contamination theory, to which Dr. Farr appealed for an explanation of the outbreak in East London, and which he has lately done so much to elucidate in his last Report,” &c.

The columns of this Review have already contained observations on the proceedings of the conference, and it is unnecessary to enter at length into any detailed account of the examples given in corroboration of the advocacy by the conference of Quarantine. We would here, however, observe that too much trust has been placed upon some of the reports submitted to the Conference, which, indeed, does not ignore their imperfect nature. Every one familiar with the East knows that frequently reports afforded by officials in many, especially of the smaller towns, are inaccurate.

We would draw the attention of the conference, especially of

the French members of it, to the evidence of their own countryman, than whom we should suppose no one could form a better or more impartial opinion, M. Cazalas, Inspector of the Sanitary Department of the French Army, who, having fully inquired into the question of contagion or non-contagion of cholera, declares that it is not contagious; he consequently does not advocate quarantine, but the institution of an international sanitary code. Our own opinion, as far as it goes, does not exclude contagion, as one fact, as Dr. Payne expresses it, in the propagation of the disorder. But our experience, extending over two epidemics in the East and three in the United Kingdom, induces us to think with Cazalas, and very many others, that quarantine is not efficient against cholera, but rather sound sanitary measures; foremost of these we may take, on the authority of Sir William Jenner, to be, as before quoted, good water.

Dr. Payne having carefully examined the several instances given by the conference illustrative of the value of quarantine writes thus:—

“Summarising the evidence which we have now brought forward, and stating for convenience of application the conclusions to which we have been led, we arrive at the following propositions:—

1. *Quarantine being a mechanical measure, and addressed only to the prevention of personal communication, can, in the nature of things, operate only so far as this process contributes to the diffusion of disease.*

2. *The particular elements of quarantine practice to which it has owed its success can be, and have been, applied with full effect in places other than quarantine stations, without any of the attendant evils of such stations; and that, therefore, where the success of quarantine is apparent, it affords no argument for the maintenance of the system.*

3. *Whether personal communication contribute much or little to the spread of cholera, there are wider agencies at work in determining epidemic diffusion, which, though they are not yet definable, are certainly of such a nature that no process of personal restriction can counteract them; and although such agencies are beyond those comprising the known conditions of insalubrity, there is no room to doubt that, within the limits of insalubrious places, crowding and mismanagement may give to the results of importation the proportions of an epidemic.*

4. *Whatever truth there may be in the doctrine that cholera is communicable from man to man, this property, though it may determine the occurrence of first cases in a place, has little or no further share in producing an epidemic. It is inoperative in the presence of good sanitary management, and may be effectively opposed by measures applied to cases of actual disease. It does not, therefore, support the policy of quarantine, nor extended restriction of intercourse.*

5. *Bodies of men arriving from infected places, whether there have*

or have not been among them evidence of infection, have suffered after arrival, in more or less degree, exactly as the sanitary condition of the place of arrival would render probable.

6. *Where this condition has been favorable to immunity disease has been arrested; where it has favoured its increase it has increased and spread, whether the cases of imported disease have been many or few, severe or mild.*

7. *The same circumstances have determined the degree of infection of towns and neighbourhoods after choleraic arrivals, whether general isolation has been completely or incompletely enforced, or has not at all been attempted.*

8. *From the foregoing propositions a general law becomes evident, that groups of persons arriving from infected places share the fate of the residents and neighbours of the place of arrival, with regard to the risk they incur from communication with actual cases of cholera. The few exceptions to this law which, as a matter of fact, are on record, viz. the instances in which arrivals have suffered severely after landing without spreading infection, are those, and only those, in which they have been themselves unfavorably situated in the neighbourhood of more favoured places; and when towns have been severely visited, without corresponding intensity of disease in quarantine stations, it has been shown that there was special care of sanitary matters in the latter.*

9. *In a large majority of the examples in which the efficacy of isolation is affirmed, it is clearly provable that isolation cannot have had any influence on the events, and in no single instance has its necessity or usefulness been demonstrated.*

10. *Upon the general question of communicability of cholera from person to person the conclusions set forth by the Conference must, for the present, be received with the utmost caution, for they rest mainly on the relative dates of imperfectly known events; and in those cases in which we are in possession of detailed information from other sources, the records of the conference are contravened in points very material to the issue.*

Dr. Abbot's work, alluded to in some of the foregoing pages, is simply a translation of the Report to the International Sanitary Conference of a commission from that body on the origin, &c., of cholera, and contains no original observations.

We may here observe that much interest attaches to Lyons, on account of its exemption from cholera.

The readers of the Review are already familiar with Dr. Pacini's name in connection with the present subject. His work has been recently alluded to in the Review;¹ want of space obliges us to postpone the consideration of it.

Concluding remarks.—Almost all writers agree that it is to prevention we must chiefly look for success in dealing with cholera, and, as we have on a previous occasion stated, this

¹ April, 1869.

dread disease seems, after all, not without its good to mankind ; indeed, we might add, to the lower animals also, for by its great importance as an awful visitation, it has forced upon our attention the fearful consequences of neglecting sanitary matters, more especially in reference to water ; and in this way it has tended to the elucidation of the agency whereby, it seems, other diseases—typhoid fever, for instance—are engendered.

The great importance of treating the diarrhoea stage of the disease, which attention so generally detects as the early phase of the disorder, is now again dwelt upon by many trustworthy observers.

It should also not be forgotten—a point to which our French brethren have, in common with some of our own countrymen, drawn attention—that irregular action of the bowels, constipation, &c., as well as other states of ill health, very frequently precede the further development of the malady.

Any departure from a state of health then becomes doubly needful of attention when cholera prevails epidemically.

Lastly, though observers may differ as to what constitutes the best principles of prevention to be adopted in connection with so grave a disease, we willingly pay our tribute of admiration to the energy displayed by our French neighbours in the matter, in reference to the East ; having said so much, we trust they will excuse our advocating the adoption by themselves of measures for the prevention of the disease in every part of France, especially wherever it showed a partiality by appearing in more than one season consecutively lately.

In the United Kingdom we would strengthen the hands of those who insist upon clean air, earth, and water, for every one, as the first essentials for security against cholera.

With regard to the fungus theory of the disease, we believe nothing has as yet been accomplished with certainty.

IV.—*Meteorological Journal.*¹

A FEW months ago we felt justified in expressing our unqualified approval of the work already done, and of the further objects held in view, by this useful Society. The favorable opinion we then formed has in no wise been altered by a longer and, we may say, closer acquaintance with it, the representative body of meteorological science in Scotland.

¹ *Journal of the Scottish Meteorological Society.* New Series, Nos. XXI, XXII. January and April, 1869. Pp. 64. Quarterly. Edinburgh and London.

From a glance at the contents of the numbers of the journal at present before us, it is evident that the association is advancing, *pari passu*, with the science whose name it bears, and whose exponent it justly claims to be.

That meteorology is still in a crude and imperfect state cannot be denied ; that, however, it is making rapid progress is no less certain. And a most convincing proof of this assertion is, that of late meteorologists have ceased to depend on observation alone as a means of extending their knowledge of weather phenomena, and have ventured to avail themselves of the equally valuable results obtainable from experiment. It is through these two agencies, observation and experiment, that every science is perfected. The former element first comes into play, the latter afterwards tests and proves the results obtained therefrom. When, consequently, we see the aid of experiment largely drawn on in the pursuit of meteorological inquiries, it is an earnest that progress will be made, and that we may hope at no distant period to arrive at accurate and trustworthy conclusions.

In the twenty-first number of the journal we have illustrations of the advantages derived from both observation and experiment. Of Mr. Buchan's paper on the 'Mean Monthly and Annual Pressure of the Atmosphere in Scotland,' we may truly say, "*Finis coronat opus.*" The labour of arranging, comparing, and drawing deductions from observations taken twice daily, during a period of eleven years and at fifteen stations, is well rewarded by the satisfactory results obtained. A subject which was hitherto involved in much obscurity has been simplified and explained, while important laws of a general and trustworthy nature have been developed and laid down. We give the principal of these as briefly as may be :—

(1) The mean atmospheric pressure *of the year* increases from West to East over the whole of Scotland.

(2) The pressure also increases uninterruptedly from North to South.

(3) Among the *monthly* pressures, there are three depressions and three elevations, the depressions occurring in January, March, and October, and elevations in February, May, and November.

(4) A remarkable barometric gradient from south to north exists over Western Europe during the months of June and July.

The importance of these laws is recognised at once, when we remember that on barometric pressures depend the directions of the wind, variations of temperature, and of the hygrometric state of the air, the quantity and frequency of the rainfall, and, in a word, all the meteorological phenomena which exercise so great an influence on the hygienic state of a country.

A group of papers in the same number, having reference to the puzzling subject of "ozone," bears testimony to the use of experimental researches. The first of this series is the report of a committee which was appointed in 1868 to carry out experiments as to the nature and properties of ozone, and as to the best method of observing it. In the second communication of the group, Dr. A. Crum Brown gives an interesting *résumé* of our knowledge of the same substance under the five following heads:—(1) The nature of ozone; (2) the sources of ozone; (3) the actions of ozone; (4) the tests for ozone; (5) the modes of applying these tests.

Of the tests for ozone, that which depends on its action on iodide of potassium seems preferable to all the others. To this test there are, however, several objections—first, it is asserted that both chlorine and nitric acid may produce a similar action to that of ozone; secondly, since in the application of the test some iodide of potassium and iodate of potash are ultimately formed, while some iodine is removed by evaporation, "it is plain that the amount of *free iodine* is no measure of the quantity of ozone which has acted."

The first of these objections does not appear to have any foundation in fact, and Dr. Crum Brown shows in his paper how the second may to a large extent be obviated.

Another objection to the same test is removed by Dr. Arthur Mitchell's proposal set forth in a 'Note on Ozonometry,' by that indefatigable worker.

The objection is that, as different quantities of air pass over the test-paper in equal periods of time, the accurate estimation of the quantity of ozone present in a given amount of air is rendered impossible. Dr. Mitchell proposes to remedy this defect by the use of an *aspirator*, which causes a given amount of air to pass over the test-paper, inclosed in a box and protected from light. Diagrams of different contrivances to effect this object are also given in the paper.

In the twenty-second number (that for April, 1869) a long article on a subject which is now demanding much attention, namely, the 'Temperature of the Soil compared with that of the Air,' occurs. This is an inquiry which, though most intimately related to agricultural interests, is yet of importance from a medical standpoint. Indeed, all the meteorological conditions of the soil are worthy of investigation by members of our profession, and it is a matter of no small reproach that our continental neighbours are far in advance of us in all such researches. To prove their importance, we need allude only to Professor Pfaff's communication on the relation between the 'Water of the

Atmosphere and the Soil' (*Zeitschrift für Biologie*),¹ and to the subsequent investigations made by various German physicians on the relation between typhoid fever and the height of the underground water.² In the latter essay there is evidence of a light, dim, however, as yet, struggling to break through the impenetrable mystery which veils from us the nature, and modes of propagation, of various diseases. May we not hope that, as a consequence of further observations of the kind indicated in the above papers, the nature of malaria, too, may some time be demonstrated?

A report on the 'Meteorology of Iceland' has led to the elucidation of some useful facts connected with the climate of the British isles. The barometric curves, in particular, which have been observed in that "ultima Thule" are proved to exercise a very great influence on our weather variations, and the advantages which would be secured by telegraphic information from Iceland cannot be overrated.

An account of a novel way in which an institution like the Scottish Meteorological Society may be of use to all classes of persons in private life is given by Mr. Buchan under the head of 'Cases of Inquiry for information at the office of the Society on matters affecting important interests.'

We cannot forbear to quote one of these, on the principle: "ab uno disce omnes":—

"Observations showing the particular character of the weather on a day in November, 186—, were asked for. A gentleman of weak mind and in infirm health was boarded with some friends who had been directed by the medical attendant on no account to allow him to walk out in wet or cold weather, such weather being, from the nature of his complaint, almost certain to prove fatal. On that day he had been allowed to go out. Shortly after he took ill and died, and the friends with whom he resided at the time thereby succeeded to considerable property. A lawsuit was instituted by some other of the relatives, who stated that the day on which the gentleman had gone out was wet and cold, and that he had been allowed to go out in this weather for the purpose of bringing on illness. An examination of the returns from two of the Society's stations in the immediate neighbourhood, together with others at a greater distance, showed that on that day in question the barometer was moderately high and steady, the temperature a little above the average, the wind very light, the sky clear or nearly so, and no rain; in other words, that the weather was indicative of a fine November day."

In addition to the papers alluded to above, tables of observa-

¹ Translated by W. D. Moore, M.D., from the "Ugeskrift for Læger," 'Dublin Quarterly Journal,' August, 1869.

² 'Dublin Quarterly Journal,' May, 1869.

tions on the climate of Scotland for three months are given in each number of the journal. The exhaustive and reliable character of these weather reports will be seen from the fact that there were in January last eighty-nine stations connected with the Society, of which there were eighty-six in Scotland, two in Iceland, and one in Faroe. Besides this, reports are regularly received from the following places, viz.—Cyprus, Jerusalem, Damascus, and Yanina, in Greece.

When we state, in conclusion, that the Society is entirely self-supporting, we fill up the meed of praise which is so justly its due.

V.—Sir James Clark's Memoir of Dr. Conolly.¹

It may possibly be thought that the pages of this Review are not a very suitable place for the notice of a biographical memoir; but there are circumstances which make this memoir of John Conolly exceptional. For, in the first place, Dr. Conolly was *one* of the original editors of the 'British and Foreign Medical Review,' which review may be called one of the parents of this work; to him, conjointly with Sir John Forbes, we owe not only its existence, but its characters and principles. In the next place the memoir contains a record of one, among many others, of those social and national reforms which the public owe to the medical profession; and if these are not reasons enough, it is well known to all of us how long and carefully the biographer has watched over the honour of our profession, and how, by his own unostentatious career, he has added greatly to its status and dignity. Any memoir of one of his old friends, or of his early professional life, any insight into the early aims and aspirations of those who now must be looked upon as the fathers of the profession, will be read by all of us with the kindest interest and sympathy.

Sir James Clark's book is, however, scarcely a memoir in the ordinary acceptation of the term; it is less, or perhaps only secondarily, a memoir of the man, but is chiefly a history of what he did. We get an insight into Dr. Conolly's mental character in the course of the history of his labours, and as it were incidentally, and perhaps this is the truest and it certainly is the pleasantest sort of biography.

Dr. Conolly's grand work, his mission, was, as we all know, the establishment on a sound basis of the non-restraint system of treatment of insanity. The system has been everywhere well dis-

¹ *Memoir of John Conolly, M.D., D.C.L., comprising a Sketch of the Treatment of the Insane.* By Sir JAMES CLARK, Bart., K.C.B., M.D., &c. London, 1869 Pp. 248.

cussed, and very frequently in these pages, and this memoir was not required to elucidate the principle of the system. One may find, however, scattered through Sir James's book different expressions of opinion of many writers in regard to it. The best definition of it, and in frequent conversation with Dr. Conolly we know that view coincides with his, is, that the principle of the system consists in the production of a proper mental effect on the patient's mind; it consists in the influence that the sane mind can exercise upon the insane; and since mechanical restraints, punishments, threats, and such like produce irritating effects upon the mind of the patient, they are on that account contra-indicated: while the treatment by restraint, as enumerated by its own advocates, consists in intimidation.

Sir James Clark produces abundant proof of the beneficial results of the humane treatment which has now been established in this country for thirty years, and it is to be hoped will never be reversed, though we learn and we know that Dr. Conolly, in moments of depression, during his last illness, sometimes would give expression to fears on the subject.

"A desponding state of mind to which Dr. Conolly was at times subject, led him to fear lest the selfish interests of mankind might lead to a reaction, and that much of his work might after all be lost to the world. It seems to have been in some such mood that at the conclusion of his last work he was led to express an anxious hope that 'nothing should be allowed to interfere with its continuation, that no future economy and no delusive theories would ever lead to the abandonment of non-restraint in the treatment of the insane. The system,' he well adds, 'as now established, will form a not unimportant chapter in the history of medicine in relation to disorders of the mind.'"

Sir James Clark gives an account of the spread of the system of non-restraint in different countries. Appendix A—"Is an account," Sir James writes, "of the condition and management of some of the principal asylums on the continent of Europe furnished me by my friend Dr. Arlidge." In this account of the condition of the asylums, those of France, Italy, and Germany, which were visited by Dr. Arlidge, were found to have many serious defects, and restraint continued to be practised (with but few exceptions) in all. While the chief fault attributed to the English asylums is their unwieldy size. The spirit of evil, thwarted in one monstrous mischief, has been at work in a fresh direction to nullify the good, the philanthropic exertions of Conolly effected!

"It would be fortunate for this country and its insane population," says Dr. Arlidge, "could the visiting magistrates of asylums

be induced to listen to the concurrent opinions of medical men, and to learn that the rational object of collecting insane people together is not safe custody, proper feeding and clothing, but cure."

It would, indeed, be very fortunate if those magistrates who continue to heap lunatics on lunatics would take advice on any subject from any extraneous source. Still it is a gratifying thing to learn from these documents that the system of non-restraint is gradually extending over the continent, and that in England there exists a strong bulwark against a relapse to the old cruelties and ignorance in the present Commissioners in Lunacy and superintendents of public asylums.

Such is all our space will admit of with regard to Dr. Conolly's work which the memoir deals with. But—

"When the grave has closed over a man the greater part of whose life has been devoted to works of benevolence, which have conferred a lasting benefit upon mankind, it is due to his memory that the public should be reminded of what he did, and of how he achieved his high objects."

These words form the opening of Sir James's preface. We can say no more here of what he did, but in reviewing what and how he achieved it we shall get a better insight of that which is the more interesting part of a biography—the mental character of him upon whom the grave is closed. There is no attempt in the memoir at an elaborate analysis of Dr. Conolly's character; no desire shown, as it were, to anatomise his soul, or to lay bare, by a process of minute dissection, every nook and crevice in his human heart. Sir James rather paints with free hand a picture from nature, without apparent design as to the ultimate effect. We gather from expressions casually happening in the narrative many little traits of character which those who knew Dr. Conolly will recognise at once as scraps from nature, such as in the preface: "My long and lasting friendship with Dr. Conolly, and my natural desire to do justice to his memory, were sufficient incentives to make me undertake the task." "My own part in the preparation of the memoir has been a very agreeable one. I can honestly say it has been a labour of love." Similar feeling Dr. Conolly awakened in very many who had ever been much in his society.

A trait which also, as it were, crops out at numerous points is Dr. Conolly's constant desire to give due credit to all fellow-workers. He seldom, in fact, spoke in public without reference to those who preceded him. He alluded to them, in fact, on every occasion, in his annual reports, in his published works, at every public speech he was called upon to make; and these latter were not few, for Dr. Conolly was by nature sociable.

These speeches were always fluent, graceful, and kindly; again, as Dr. Browne writes (p. 190), "He was sensitive in his rectitude, gentle, and genial; he was to all men conciliating and courteous; to his friends, and I judge him after an experience of thirty years, he was almost chivalrously faithful and generous, and the insane he positively loved."

Dr. Hitchman's account, in a letter to the author, given at p. 40, depicts Dr. Conolly's manner and character in some other minor traits. Dr. Hitchman is well qualified to give a just estimate of Dr. Conolly. He is a gentleman, as all who know him will allow, calm and discriminating in his judgments, and one who would speak with fairness on the subject. Dr. Hitchman says—

"He was, indeed, a noble enthusiast in the best sense of the term. His work was a labour of passionate love, and his language, therefore, was often fervid. He had, however, in common with all men engaged in great duties, his moments of despondency. He was a great sufferer from an irritable chronic skin affection, which often deprived him of much sleep at night and irritated him during the day."

These temporary conditions of irritability, it seems from Dr. Hitchman's narration, were not very readily forgiven, or duly credited to their cause by some of those around. His irritants were unfortunately not merely directed to the dermal surface, or, as Dr. Hitchman somewhat mildly puts it, "the irritation caused him to appear impatient and excitable by officers and others, who knew not or could not appreciate the corporal conditions and mental anxieties which produced the feeling." In less considerate terms, Conolly had around him men who could not or would not make allowances for a single infirmity which they could detect in him.

After he ceased to reside at the asylum,—

"He spent the greater part of two days in each week," says Dr. Hitchman, "in the wards among the insane he loved; his interest in the patients seemed never to flag. Even cases beyond all hope of recovery were still objects of his attention. He was always pleased to see them happy, and had a kind word for each. Simple things which vainer men with less wisdom would have disregarded, or looked upon as too insignificant for their notice, arrested Dr. Conolly's attention, and supplied matter for remark or commendation; *e.g.* a face cleaner than usual, hair more carefully arranged, a neater cap, a new riband, clothes put with greater neatness, and numerous little things of a like kind, enabled him to address his poor illiterate patients in gentle and loving accents, and thus woke up their feeble minds, caused their sad faces to gleam with a smile, even though

transient, and made his visit to the wards to be longed for and appreciated."

He had, in fact, by nature, many excellent qualities of heart, fitting him for the work he was to do. The subject of insanity, too, from an early period of his professional studies had been a favorite one with him, and it formed the subject of his inaugural thesis for the doctor's degree. That he had a great power of appreciating character—a quality of great importance in all intercourse and in all treatment of the insane—is shown in that elegant and charming essay, the 'Study of Hamlet.' His power of making himself agreeable to every one, his easy and graceful fluency in conversation, were all excellent adjuncts in securing for him the success he achieved. One of the most remarkable things, however, in the whole history of Dr. Conolly's work is the short space of time he took to carry it out, and to establish it on a sure and firm basis. The affair was a veritable coup. Ten years is a short space of time in the history of common wars, in which only strength of limb and length of purse are required; but to complete a victory over prejudices which had formed part of men's settled principles for ages, viz., that the lunatic was to be cowed, coerced and frightened into subjection; that burly keepers and stern-looking athletics, with iron bars, bolts, and straps, were the only things possible to accomplish the end, was a task of quite a different kind. Conolly had to reverse the order of things. He had to train, educate, and convince all manner of opponents, and this single-handed.

Surely such a complete social revolution was never before accomplished in so short a space of time, though there is no doubt that a considerable feeling against the old system of things had existed for some time, and was increasing—a milder coercion, that is, was beginning to be generally practised. In 1792 or 1793 Pinel unchained the lunatics at Bicêtre. In 1796 Tuke, of York, introduced a milder system of restraint than had ever been before practised in this country. In 1815 public opinion was aroused about lunatic asylums. Attention had been forced on the sad state of old Bethlehem—"Bethlehem, with its chained rows of raging lunatics, the men on one side, and the women on another side, had been a Sunday sight for the Londoners." In 1818 or 1819 new asylums were built, and the improvement became more general.

Dr. Charlesworth, the visiting physician of the Lincoln Asylum, was particularly prominent in lessening the severity of restraint. His resident medical officer, Mr. Gardiner Hill, warmly and energetically supported and executed the views of

his physician, and the idea of total abolition of absolute non-restraint is due to Mr. Hill. This is to be found stated plainly in whatever Dr. Conolly wrote, and was repeated whenever he spoke on the subject.

It ought to be remembered that total abolition is quite different in principle from mild restraint. Foreign writers confuse these different principles. In whatever light we view the two systems, the difference is to be seen. In fact, it is the confusion on this point that has operated in preventing the spread of non-restraint to other countries, and even done much to nullify the good of non-restraint in this country. If we judge the two principles by the *mental* effect they have upon the patient, the difference will be evident. It is one thing to produce on the patients a feeling that unless they behave themselves they will be bound down by camisoles, like many poor fellows they see daily before them. It has quite a different effect upon the madman to see that, however irritating and provoking or insolent their fellows are to the attendants, that the attendants meet them with nothing but kindness in return; to feel that they are always spoken to in a friendly tone, and to know that the whole system of treatment is not to coerce them, but to quiet and befriend them. In totally abolishing restraint, even in its mildest form, Mr. Gardiner Hill introduced a totally different principle of treatment. Dr. Conolly's words are (p. 50)—“ In 1838 all restraint had been abolished at Lincoln; and, perhaps, had I not seen that it was quite practicable to conduct an asylum without restraint, I might not have had the courage to make the attempt.” This is, as ever, modestly expressed by Dr. Conolly; but we learn from it that, on Dr. Conolly's appointment to Hanwell, in 1839, the ground had been to some extent prepared for the foundation of his work.

Dr. Conolly's reputation and influence were such as to give him authority and weight even with those whom he met in the committee of Hanwell. It is well known that in London the magistrates, who form the committees of all public asylums, are drawn from a different class of men from those who form the same bodies in the provincial districts. The active magistrates in the provincial county asylums are the landed gentry, but at Clerkenwell Sessions House, perhaps, a gentleman with a good salary in the civil service, or a retired officer from India living near the asylum, or an aspiring barrister looking for a recorder-ship, may assist in the councils and get the patronage which the office affords; and it was to such men that Dr. Conolly had to look to for his appointment to Hanwell, and, when elected, to influence sufficiently, yet so as not to incur their jealousy or opposition.

Already they had rejected him in favour of a very different

kind of man, Dr. Millingen. The committee, and especially their chairman, took a real and warm interest in the institution in its corporate capacity, and they soon found that Dr. Millingen was better suited for a different sphere of life than physician to their asylum ; and Dr. Millingen resigned after a year of office. In his year, however, as far as the patients' treatment was concerned, the asylum was the gainer of several more restraint chairs, not that Dr. Millingen probably was favorable to any increase of restraint. The year thus lost to Dr. Conolly, however, appears to be the year in which the system of non-restraint was originated at Lincoln. Dr. Conolly entered upon his duties at Hanwell in 1839. The principle, then only commencing at Lincoln, was at once inaugurated and developed at Hanwell.

Dr. Conolly had much to support him in carrying out this system ; much more, indeed, than the support which he acknowledges to have been accorded to him by the committee, who indeed at first, and for some time, were considerably flattered by the great things which were being said of Hanwell, and by seeing that many distinguished visitors who were attracted to its wards. Still Dr. Conolly was supported mainly by his own status among his professional brethren, by those whose acquaintance he had made in other works. "He had a large and agreeable correspondence," says Sir James Clark, "with men of science and literature, arising, in a great degree, from his connection with the Society for the Diffusion of Useful Knowledge, the meetings of which he generally attended, and to which he contributed various publications. He was also engaged in literary works with his friend Sir John Forbes, chiefly in preparing the 'Cyclopædia of Practical Medicine,' a large and important work, and in editing the 'British and Foreign Medical Review.'" He had been Professor of Medicine at University College, then called the London University, during a previous part of his career, so that he entered upon his duties at Hanwell with influences of personal character to which his modesty would not allow him to allude.

Other matters, which our space here denies us the pleasure of enlarging upon, are alluded to in Sir James Clark's memoir. The interest in many other works of general philanthropy and medical science, among the former the Idiot Asylum, the educational and social movements of the day, the British Medical Association, and indeed everything which could appeal to Dr. Conolly's benevolence, never appealed in vain. "His name," said Sir Thomas Watson, "will go down to a remote posterity, and be reckoned among those of the greatest and most noble benefactors to a very suffering portion of the human race that our profession

and our country have ever produced" (p. 193). Sir James Clark's book is one which will form an ornament to any medical library; it is a record of one of the brightest acts in the medical history of our times; it records one of the greatest debts of society to pure medical science.

VI.—Researches on Leprosy.¹

DURING the last twenty years we have repeatedly devoted considerable space to the subject of Leprosy.² The problems which this disease offers to the physician are, however, so intensely interesting; so tantalising at each step, so apparently plain and yet so difficult, that we make no apology for again adverting to it, and bringing under notice such new data as are obtainable. There is, perhaps, no other malady respecting the causes of which we are as yet so entirely in the dark. Yet leprosy is common in many countries, and so far from having been neglected, it has roused the enthusiastic zeal of men like Boeck, Danielssen, and Virchow. Of Dr. Danielssen it may be said that he has devoted a life's labour to his subject, with the advantages of a most acute mind and every help which abun-

¹ 1. *Om Spedalskhed som endemisk Sygdom i Norge.* Ved CAND. MED. BIDENKAP. Christiania. 1860. Pp. 208.

On Leprosy as an Endemic Disease in Norway. By Dr. BIDENKAP. (With Map and Tables.)

2. *Beretning om Lungegaards Hospital, &c., Virksomhed i Treaaret, 1865 to 1867.* Ved D. C. DANIELSSEN, Overlæge. Christiania. 1868. Pp. 47.

Report of the Practice of the Lungegaards' Hospital for the triennial period, 1865 to 1867. By Dr. DANIELSSEN.

3. *Den Spedalske Sygdom blandt de Norske i Amerika.* Af Overlæge, J. A. HOLMBOE. Pp. 45.

On Leprosy amongst the Norwegians in America. By Dr. HOLMBOE.

4. *Tabeller over de Spedalske i Norge i Aaret, 1866, &c. &c.* ('Norges Officielle Statistik,' 1867, C. No. 5.)

Tabular Statement of the Lepers in Norway in the year 1866, &c. &c.

5. *Syphilisatinen anvendt mod Syphilis og Spedalskhed.* Ved D. C. DANIELSSEN, Overlæge. Bergen.

Syphilisation as a Remedy for Syphilis and Leprosy. By Dr. DANIELSSEN.

6. *Report on Leprosy by the Royal College of Physicians, London.* (Second Notice.)

7. *Clinical Report on Cases of Leprosy.* By JONATHAN HUTCHINSON. 'London Hospital Reports,' Vol. I, 1864.

8. *On the Symptoms and Morbid Anatomy of Leprosy; with Remarks.* By H. V. CARTER, M.D., Lond. 'Transactions of the Medical and Physical Society of Bombay,' No. VIII, 1862.

9. *Norwegian Notes.* By JONATHAN HUTCHINSON. 'Medical Times and Gazette,' August, 1869.

² See this 'Review,' vol. v, for 1850, page 171; also January, 1863, Original Communication, by Dr. H. V. Carter, and April, 1867, in 'College of Physicians' Report.'



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dance of material and liberal government aid could give. Of late years, many important points as to symptoms and pathology have received most valuable elucidation, but our position as regards knowledge of cause, prevention, or treatment, remains just where it was.

The notion that leprosy is "a skin disease" has of course, been long ago abandoned. It is now known that it is a thoroughly constitutional malady or diathesis, of which some of the most frequent results are chronic inflammation of the skin and of the cellular tissue in various parts, leading to deposit, of what for convenience sake may be called leprosic lymph. This deposit occurs very frequently in nerve trunks, enlarging them and destroying their function by pressure, and thus inducing other consequent symptoms: numbness, muscular atrophy, lesions of nutrition, &c.,—which are amongst the most conspicuous of its well-known phenomena.

Dr. Danielssen states that he has found leprosic deposits in almost every organ of the body, the kidneys alone excepted. His description of the nerve lesions have been fully confirmed and considerably extended in detail by the able researches of Dr. Carter of Bombay.

Concerning this chronic neuritis which is such an important point in the pathology of the disease, we may premise that it does not occur irregularly, that it is usually symmetrical, that it prefers certain nerves to others, affecting the sensory rather than the motor, but scarcely ever attacking those of special sense; that it is believed usually to begin near to the periphery, and to extend towards the centres. In reference to the last point, however, it is certain that it often attacks the trunks of nerves of considerable size, such, for instance, as the ulnar.¹

Our next general statement shall be that the disease is the same in all parts of the world where it prevails, or exhibits at any rate only slight deviations of type. The observations of Dr. Carter at Bombay coincide remarkably with those of Danielssen and Boeck at Bergen, and those of many other observers in most distant parts, agree also most closely, whilst the records of the past prove further that leprosy (using the word carefully) has been essentially the same in all ages. The two forms "anæsthetic" and "tubercular" are recognised by all accurate observers, as also the facts of their close relationship and frequent intermixture.

Let us now try to state the difficulties of the problem *as to cause* which this malady presents. It prevails extensively in

¹ The plates published by Danielssen and Boeck show the enlargement of nerve trunks in a very graphic manner.

some localities and amongst some communities, yet it is certainly not contagious. The places in which it is prevalent, Mexico, Norway, China, to wit, differ so greatly in position, that it is impossible to refer it to climatic influence. There is historical evidence for the belief that it was once very common in countries from which it has now wholly disappeared (England and the continent of Europe, for instance), yet to suppose that it disappeared before the gradual spread of civilisation is difficult, because some of the places where it prevails largely are just as advanced as those which have got rid of it. It is transmissible hereditarily, yet that this is not the sole cause of its persistence is proved by the fact that it may begin *de novo* in strangers who immigrate into its territories. Those who wish to realise the present position of the inquiry will do well to compare the leprosic diathesis with those of gout and tuberculosis. All three are capable of hereditary transmission; in all this transmission is seemingly irregular, and may leave many members of the family or even whole generations untouched. All three have been present from time immemorial, and are described in the most ancient records of medicine. All three may be produced in those who have not inherited them. In all, the tendency may remain wholly latent for many years or the greater part of a life. All three develop themselves, as a rule, gradually, and with slowly increasing severity. In all, the outbreak may occur at any age and in all, if it happen in childhood, inheritance may be inferred. In all, lesions of the most distinct and characteristic kind are announced by the pathologist. The similarity is, indeed, very close between the phenomena, and general course of these three different forms of diathesis. Respecting two of them, we know to a certainty how they are to be originated, and could produce them artificially at any time. Let a man drink port wine or beer freely, eat meat largely, and take but little exercise, and he will pass quickly into the gouty dyscrasia; he will gain the power of bequeathing the gouty diathesis to his children and his children's children. It is probable that he could do this more quickly and certainly in some climates than in others. The causes of tuberculosis though, perhaps, a little less easily stated, are not less definitely known. They, too, are climatic and dietetic, and the result of their influence also is when once developed capable of hereditary transmission. Respecting both gout and tuberculosis we know much as to modes of cure; we know that by attention to the patient's habits of life, place of residence, and kind of food, we can influence his diathesis; and that although, perhaps, we cannot undo what is already done, yet we can prevent further advance, and mostly diminish the risk of hereditary transmission.

As to leprosy, however, although its origin, cause, and events fit so remarkably with those of the two diseases we have mentioned, that we are forced to believe that it, like them, must have its starting-point in the influences of climate or in kind of food, no light whatever can be afforded as to what those influences are. We know nothing as to its prophylaxis, and nothing as to its treatment—facts which are sufficiently illustrated by the circumstance that its ablest investigators have fallen back upon the rude device (but possibly a very wise one) of trying to prevent its transmission by keeping its victims in asylums and preventing their marriages.¹

Another comparison may be instructively made, that, namely, with the ague diathesis, or the results of malaria. Leprosy, like intermittent fevers, is endemic; it, like them, has been banished from wide districts apparently by the spread of civilisation. Certainly very marked differences are to be noted, however, between the two. The peculiarities of an ague district are assignable, and we know how they have been removed from the parts from which the disease has disappeared. The features of the countries where leprosy prevails are most dissimilar, and nothing whatever can be said as to climatic change in the places (Iceland, Scotland, &c.) which it has abandoned. Still more definite is the fact that, where ague influences are strong, all who come within their reach suffer, whereas in leprosy districts large portions of the community live amongst it without the slightest fear of ill consequence. It is only certain classes who are liable to it, a circumstance clearly proving that it is endemic in some other sense than that it depends upon atmospheric influences. Then, again, an ague patient improves directly if he is removed from the district, and it seems probable that, instead of transmitting a taint to his offspring, he confers upon them some degree of the immunity which acclimatisation gives. The reverse of these is the fact in leprosy, and it seems quite clear that the dyscrasia of the latter offers a much nearer analogy to those of gout and tuberculosis than that of ague. Let us keep in mind the inference from this, that it is more likely to be a disease induced by diet than by any kind of malaria.²

Some of the most interesting parts of the inquiry will come out with increasing clearness if (availing ourselves of the works

¹ A bill for the compulsory attainment of this object was not long ago introduced into the Norwegian Storthing, and strongly supported by medical testimony.

² In his Norwegian notes, Mr. Hutchinson mentions that Dr. Bidenkap, one of the latest and most zealous investigators of the disease as it occurs in Norway, avowed his belief in its local origin through some telluric emanation or miasm.

which stand at the head of this article) we notice in some detail the present condition of leprosy in Norway.

The work of Dr. Bidentkap is chiefly devoted to the determination of the precise districts in which the disease prevails, the influence of hereditary predisposition, &c. He gives an excellent map illustrative of ratio of prevalence which we here, with some slight alterations, copy.

It will be seen at a glance that the disease does not occur over the whole country, but is strictly confined to the middle part of the west coast. In the south districts, neighbourhood of Christiania, &c., leprosy is as unknown as in England, and nearly the same immunity may be claimed for the north. In the district in which it is found it will be observed that its occurrence is very patchy, that some parts suffer extremely, and others but slightly. A limited district, just north of Bergen, and adjacent to the beautiful Sogne-fjord, is the blackest; next we have a long narrow belt of coast beginning a little south of Trondjem (Drontheim), and extending several hundred miles north of it. Passing further north we have after an interval another dark patch, then another interval, and then another patch, the last. Those unfamiliar with Norwegian scenery may be reminded that the population is by no means evenly distributed in its huge parishes, many of the latter consisting in almost their whole extent of masses of uninhabitable mountain. Thus, in the map the districts which are shaded dark have very probably their humanity element restricted to a few spots. We may add that the parts suited for villages are almost always near to the fjords or rivers. Inland towns are very few, and very small.

Any one glancing at Dr. Bidentkap's map, and being told that the disease which has this curiously local distribution is a very hereditary one, and that it has ceased to prevail, not only in the rest of Norway, but over almost the whole of Europe, and will, we think, be inclined to exclaim, "Then it must be endemic through hereditary transmission only. The Norwegian population is, of course, a very stationary one; close intermarriage is, probably, common and unavoidable, and hence the easy explanation of the local persistence of such a disease." That there should be climatic peculiarities (air, water, geological formation, &c.) which should belong only to these isolated districts seems, we will grant, most improbable; and with such facts before us it is impossible to feel surprise that the theory of persistence through inheritance only should have taken such strong hold on the minds of some of the best informed of the Norwegian profession. Yet there are some other facts which entirely overthrow this conclusion, and make it perfectly certain that

there still do exist some local peculiarities which are as capable of originating the disease now as they were a thousand years ago. We allude, of course, to the undoubted fact that strangers residing long in the districts may acquire it. We grant that this is not done easily nor frequently, but it occurs sufficiently often to prove the point. In further proof that it is not a mere matter of race or family, but a true endemic disease due to local causes, we have also certain statements which favour the belief that it is capable of benefit by change of residence, and that on complete change of country and of habits it will die out. Again we ask, what is this local cause? Shall it for ever elude us?

The *de novo* origin of leprosy is immigrants into districts where it prevails and its amelioration or extinction in emigrants from them, are facts of such extreme importance in the inquiry that we shall here devote a little space to the investigation of each.

The only trial, on any large scale, of the effect of emigration upon leprosy patients is that which is now taking place into the United States. It is true that in not a few isolated instances the subjects of leprosy have tried change of climate as a remedy; but this has generally been done in a late stage of the disease. Within the last few years, however, a large exodus of the Norwegians to America has taken place, and has naturally comprised many individuals who, although still in tolerable health, had a leprosy taint, either active or latent, in their blood. So important has this subject seemed to the United States Government that we are informed that measures have been adopted to discover leprosy subjects amongst newly arrived immigrants, and procure their deportation back again. In spite of precautions, materials enough have been supplied for a very instructive experiment, and the result of it is reported by Dr. Holmboe in one of the pamphlets before us. Dr. Holmboe is a native of Bergen, where at present he holds the post of surgeon to the General Hospital. He is, therefore, thoroughly familiar with the subject. In 1863 he made a tour through those parts of North America (Illinois, Iowa, Minnesota, and Wisconsin) to which the Norwegians chiefly go; one of its objects being to ascertain whether leprosy was prevalent among them. The results of his inquiry are given in detail, but we must content ourselves with citing his conclusions, which are stated as follows:

“1. There are amongst the Norsk population in North America no instances of leprosy attacking those born in the country.

“2. There are amongst the Norwegians who have immigrated, taken altogether, not a few who are now the subjects of leprosy.

“3. Most of the leprosy patients were already leprosy when they came over.

"4. In not a few cases the leprosic symptoms have had their first outbreak in America, This has happened, however, to those who were adults at the date of their immigration, who had before coming over lived in such a manner as to predispose to the disease, and who also had not profited to the same extent as most in the general advantages which life in America offers.

"5. Leprosy when imported into America has, as a rule, a longer and milder course than in Norway, and shows also a stronger tendency to spontaneous improvement and recovery.

"6. Life in America will in general avert the outbreak of leprosy, so that many will there remain wholly free who, according to all estimate of probabilities, would have suffered had they remained in Norway.

"7. Climate has certainly a great share in effecting this change. It is not nearly so rigorous and inclement in America as in Norway, and thus does not make the same demands on the vital powers of resistance.

"8. It is probable, however, that the altered mode of life has quite as large a share as climate. The immigrants are, in a general way, far better off than they were in Norway, and others are not under the necessity of exposing themselves so much to the influence of climate, whilst, when the need for exposure occurs, they are better protected.

"9. Leprosy will disappear amongst the Norwegian population in the States, or will be found only in isolated imported cases. It will not be transmitted from generation to generation as an endemic disease."

Such are the opinions of a highly qualified investigator on this important subject. Even if a little sanguine, they are, probably, near the truth; at any rate, they have not as yet been met by any evidence on the opposite side. Their value, as indicating that the duration of the leprosy diathesis under a favorable change of conditions is not without limit, is very great.

The proof *that leprosy can originate in strangers* is yet more important than the evidence as to its decline under opposite conditions. Not only do we want accumulated instances of this, but the most searching scrutiny should be made into the conditions under which they occur. We cannot too strongly urge upon all who have opportunities, the duty of recording most critically and in full detail all facts of this kind. It is by study of the conditions of origin that we may hope at length to detect the cause. As yet far too little attention has been given to this matter.

The report of the College of Physicians contains a good many rather vague allusions to the occurrence of the disease amongst strangers, but there are no definite statements as to period of residence, circumstances of life, &c.

Mr. Erasmus Wilson, in the same document, publishes no fewer than nineteen cases which had come under his observation in English practice. In each case the patient had lived in a leprosy district, but eighteen of them were of European birth. Of these ten were from Hindostan, one from Ceylon, five from the Mauritius, and three from the West Indies. His report is most valuable. The number of cases of this kind is probably far greater than have come under the notice of any other observer. In 1864 (three years before publication of the College of Physicians' Report) Mr. Hutchinson had published the details of two cases in which Englishmen who had resided in dangerous localities became the subjects of leprosy. One of these is of especial interest,¹ in that its subject, a Scotchman, the captain of a Barbadoes trader, had never lived on shore, but had merely sailed to and from the leprosy district. In the same report another case is given, from notes by Mr. Curling, in which a woman, born in Hereford, married at twenty-one, and accompanied her husband to the West Indies, where ten years later she became the subject of leprosy. In his *Norwegian Notes*, just published, Mr. Hutchinson refers to several other examples of the same event mentioned to him in conversation by surgeons on the spot. Curiously enough, the Norwegian literature of leprosy, which is copious, is very deficient in records in this point, and we may infer that such events are rare. In spite of these facts, which prove the origin of leprosy in strangers to be possible, it is undoubted that Europeans in India, China, and Mexico, feel little or no fear of contracting the disease, and that, excepting as regards the wet-nursing of children, they adopt no precautions. The same confidence in immunity is felt by all the better classes in Norway, even in the districts where leprosy is most prevalent. Mr. Hutchinson relates that one of his Norwegian informants mentioned to him the case of a German officer, who came to live in the Bergen district, and became leprosic. "But," said the narrator, "the fellow degraded himself—if it were possible for a German officer to degrade himself²—he took to drinking, and lived as our peasants do, and he earned the disease he got." Such is the creed we believe of all Norsk observers:—the disease will not attack you so long as you observe civilised habits of life. That there are, however, exceptions to this, we have already seen.

In the report of the Royal College of Physicians is given (at page 3) by Dr. Benson, the following statement:—"The disease appears to be purely hereditary, as it has not extended beyond

¹ See 'London Hospital Reports,' vol. i, page 23.

² An ebullition, we suppose, of Scandinavian patriotism.

the French people except in one case; a Scotchman, named Stewart, living seventy miles from Tracadie, had the disease, and his descendants are likewise affected. No cause can be assigned how he got it." This fact is most important, inasmuch as New Brunswick is the only part of British North America where the disease is known, and it is believed to have begun there within a recent period. Another paragraph of the same report (page viii) informs us that it was first observed in 1815, amongst the French settlers, in Tracadie, a district in the county of Gloucester, bordering the bay of Chaleurs, in the Gulf of St. Lawrence, and it is almost exclusively limited to this and to one or two adjacent districts on the north side of the Miramichi River. It is believed to have been introduced into the province by an emigrant family from St. Malo, in Normandy. Here, then, we have the question as to importation or local production put in a tangible and precise form. The circumstances attending its appearance in New Brunswick ought to be most carefully investigated. Has it spread to any others of the French, excepting the lineal descendants of the St. Malo family? How did the Scotchman get it? It is clear that either hereditary transmission, contagion or local causes of origin must have been at work in him. Dr. Gordon, another observer from the same district, gives us information of a kind which almost answers the question. He writes: "It does appear to be propagated by hereditary transmission, yet not entirely so, as individuals of different races, living in the same house with lepers, have become infected and died of the disease." Are we then to believe that the end of the riddle is that the disease is contagious?¹ To do so would be to throw aside an enormous mass of negative evidence;—evidence of a kind and quantity which has proved convincing to the College of Physicians' Committee, and the practical conclusion from which has been adopted by our government, and is now acted upon. We repeat, New Brunswick is the place where the battle as to the origin of leprosy should be fought out. That it came from St. Malo is improbable, because the disease has, we believe, not existed there within the last century, or perhaps much longer. That it has been transmitted by inheritance is probable enough, but that this has been its sole means of spreading is contradicted by testimony, not we admit quite precise enough, but still very strong. That it has arisen *de novo* in local conditions seems

¹ This was, indeed, the conclusion arrived at by a medical commission appointed by Sir William Colebrook, the Governor, twenty years ago. An act was passed empowering the erection of a lazaretto, and the compulsory segregation of lepers. The poor creatures fled to the forests rather than be imprisoned for life.

very probable, and that the causes which gave it birth in 1815 are still at work, is exceedingly likely. We may just add, that the habits of the New Brunswick leper-classes appear to be a very exact repetition of those of the fishing-peasants of Norway. Exposure to inclement weather, insufficient protection, both in the house and in clothing, neglect of personal cleanliness, a poor diet, consisting largely of fish, and that often eaten in a state of decomposition, are facts to which all observers bear testimony in both countries.¹ From amongst these several peculiarities of life, who will pick out for us the one which does the deed? That leprosy is a result of all combined is very improbable, for the disease is special and peculiar in its character, very local in its occurrence, and by no means coextensive with low civilisation. In the fear of unduly extending the present article, we will next notice, separately and very briefly, sundry points on which the works before us give special information.

Are leprosy and syphilis in any way connected? The all but unanimous reply to this query on the part of those who are familiar with the subject is, "No, not in the least." They may occur in the same subject, or they may be, and often probably are, mistaken for each other, but as constitutional maladies they are perfectly distinct, and there cannot be the slightest doubt that they arise from very different causes.

Dr. Danielssen, of Bergen, having persuaded himself of the efficacy of syphilisation in syphilis, and having by long experience been compelled to admit the almost hopelessness of leprosy, thought himself justified in trying the effect of syphilis-inoculation in a certain number of the subjects of the latter. Twenty-three patients in the Lungegaard's Hospital were syphilised. In one the disease was arrested, but the arrest was not, in Dr. Danielssen's opinion, in connection with the treatment. In all the others the disease remained unaltered, or even advanced, during the time the patient was under the influence of syphilis. The experiments have, of course, been abandoned. The evidence as to the totally distinct natures of syphilis and leprosy is so strong, and the difficulties in diagnosis in some cases confessedly so great, that the advocacy of the opinion of their identity ought to invalidate the facts supplied by any observer, as furnishing proof that he has not mastered the difficulties referred to, and has probably made many mistakes as to what was real leprosy and what otherwise. We say this from no wish to prematurely close discussion on this point, but simply as a practical

¹ Although we have said that the disease in New Brunswick is worth further investigation, we by no means disparage the reports furnished by Drs. Gordon, Benson, Nicholson, and Bayard, to the College of Physicians, which are, indeed, very ably done, and reflect great credit on those observers.

hint in employing the very voluminous mass of evidence which has been collected, large portions of which are probably valueless and misleading.

Is leprosy on the increase? The very careful statistics published by the Norwegian Government show an apparent increase there. It may be that it is apparent and not real, for, no doubt, each year the cases are more accurately enumerated, slighter cases are diagnosed and counted. Probably the statistics of twenty years ago were considerably below the mark.

In most parts of India the disease is represented as either stationary or increasing. In some localities it is, however, decidedly diminishing. Throughout the South of Europe, Italy, Spain, Palestine, &c., we believe it is decidedly decreasing, but the data are far from accurate. The evidence of Dr. Fiddes, from Jamaica, is singularly interesting and important. He writes:—

“I have every reason to believe that it has been progressively increasing in this city (Kingstown), and in the island generally, during the last twelve or fifteen years. The fact is well known to the public and to the profession, so far as Kingstown and other towns are concerned. This increase may, probably, be due to a variety of causes, of which hereditary transmission may be one of the most important, though, perhaps, the chief exciting and predisponent causes are to be found in the retrogression towards barbarism among the bulk of our population.”

Dr. Fiddes' opinion as to cause of increase is, however, contradicted somewhat by a fact mentioned by Dr. Bowerbank, another observer in the same island, who states—“The cleanliest and the dirtiest appear to suffer equally. The Jews, who are very cleanly in their habits, appear to suffer most.”

As a general statement, it is probably true that in many parts where leprosy has for long prevailed its causes are gradually ceasing to exist, whilst in many others they are still in full vigour, or even gaining strength.

Is any treatment of avail? Concerning drug treatment, almost every one agrees that we know of nothing which is of any definite use. Iodide of potassium, praised by a few, is entirely discarded after patient trial by perhaps the best authority in the world on the question, Dr. Danielssen, of Bergen. The fact that this observer has reduced his active treatment almost entirely to counter-irritation over nerve trunks, speaks for itself as to the disappointment which he met with in other directions. That improved conditions of life, especially of food, are beneficial to lepers all agree, but there are but few who assert that they will do much towards erasing the disease. It is strange, but we cannot find any evidence that the absolute disuse of fish

has had a trial by any one, so that we presume that Mr. Hutchinson's opinions (as expressed in Vol. I of the 'London Hospital Reports') have not appeared plausible to any of those in familiar contact with the disease.

We will conclude with a few words as to—

Speculations as to cause. Of the 250 replies received through the Colonial Office by the College of Physicians, we find scarcely any which convey the expression of a definite opinion as to cause. Or rather, perhaps, we ought to say that almost all agree with the Norwegian observers in regarding leprosy as a disease consequent on social degradation, poverty, and poor food. Mr. Erasmus Wilson thinks that he detects a resemblance to the diseases which arise from specific animal poisons. Mr. Hutchinson, as we have just mentioned, inclines to associate it with the use of a fish diet. As far as we understand his views, he would regard leprosy as a kind of fish-eaters' scrofula. There is certainly a great deal to be said in favour of this opinion, but also much against it; and whether or not it is more than an ingenious crotchet remains to be proved. It is true that leprosy is most common on the coast, but it formerly prevailed over the Continent of Europe, and still does so in the inland districts of India. In some of the latter observers state that it occurs amongst people who do not eat fish. We trust, at any rate, that due caution will be displayed in urging an opinion which may be without foundation, and which might easily have the effect of exciting prejudice against one of the not over-abundant means of human sustenance, and of interfering with an important branch of industry. The disease is, however, a very important one, very widely prevalent, and very dreadful in its results to those affected. Respecting such a scourge, it can be scarcely needful to insist that any plausible suggestion ought to claim our respectful attention.

VII.—Poor Law Report.—Ireland.¹

THE administration of relief to the poor is daily becoming a more and more serious question, not alone in a financial point of view, and regarded in the relationship of the claims the poor have upon the bread-winners, who have but too frequently more than they can do to meet the demands of their own immediate families, but also in regard to the influence for evil exerted upon the health of the community by tramps and vagrants. The evidence given in reference to this latter point by impartial ob-

¹ *Annual Report of the Commissioners for Administering the Laws for the Relief of the Poor in Ireland, &c., with Appendices.* Dublin, 1869.

servers is too clear to allow of the existence of doubt as to the frequent spread of contagious diseases by means of beggars and tramps, to say nothing of the crime and immorality, and consequent disease, and many other evils resulting from the encouragement too often given them by unreflecting persons, acting under the idea of relieving distress, so constantly feigned. The report before us contains most important evidence upon the encouragement and support afforded by outdoor relief to such most undeserving classes. We will place before our readers some of the statements of the Commissioners on vagrancy, and its relations to the public health.

“In the Appendix will be found a circular letter, issued in November, 1868, to each board of guardians in Ireland, on the subject of professional tramps and vagrants who seek workhouse relief usually for one night only, and pass from one union to another, availing themselves of each as a lodging-house on their line of travel through the country. We use the word ‘professional’ as distinguishing this class of tramps from *bonâ fide* applicants for temporary relief in cases of destitution not incurred by any immediate voluntary act.

“This circular has had the effect of promoting a more general enforcement of the Irish Vagrancy Act against those who violate in various ways its enactments. The law itself is somewhat different from the vagrant law in England; but from the time of its enactment, in 1847, it has not been put in force very generally, or very effectually, against the above-mentioned class of offenders. An increased pressure from this class on the union workhouses, especially those on certain lines of road, has been recently observed, and has led us to call the attention of the boards of guardians throughout Ireland to the means which the law had placed at their disposal for the repression of vagrancy.

“The casual ward, it may be observed, is an institution not yet known in Ireland; and whenever a board of guardians has proposed resorting to such an expedient we have always opposed ourselves to its adoption. However repressive of vagrancy such a system, carried out universally, may be considered to be in England, we believe that its adoption in Ireland would tend to systematise the practice of vagrancy, and afford facilities for its pursuit as a means of living. Reliance, therefore, has hitherto been placed upon the application of the same rules to this class of applicants as those which regulate the treatment of other persons when admitted into the workhouses; that is to say, the removal of the clothes on admission, the bath, the workhouse dress, and the task of work, when practicable, before discharge.

“In England, as well as in Ireland, it would appear from the poor-law returns that at periods when there is most out-door relief there is also most vagrancy.

“The system of workhouse relief, the ordinary form of relief in

Ireland, tends to diminish mendicancy and vagrancy, inasmuch as persons so relieved have all their wants supplied, and cannot be abroad begging.

Out-door relief, on the other hand (whether English, Scotch, or Irish), fails to supply all the wants of the recipients, and a large number of them, therefore, being at liberty to go where they please, naturally supplement their means of livelihood by wandering abroad and begging.

“In quitting this subject we may observe that the connection between vagrancy and mendicancy is universal, and that the great means of suppression of both is the withholding of alms, and rests, therefore, in the hands of the public itself, and in the exercise of individual discretion on the part of every member of society.

“At the same time it is proper for those who administer the laws in Ireland to see, as far as in them lies, that the enactments which recognise and punish mendicancy and vagrancy as criminal are enforced against wilful offenders.”

The system of removal of paupers who for the whole of their working days contributed to the wealth of England, Scotland, or Wales, back to Irish workhouses, if they themselves or their husbands or fathers happened to have been born in Ireland, demands redress. This subject is treated of by the Commissioners in the present as well as in preceding reports; details of cases of great injustice are given, and we must add our opinion and desire to theirs, that perfect reciprocity should exist, or better still, as they add, would it be that the laws of removal from one side of the Channel to the other should be abolished altogether.

Cases have come under our notice, in addition to those mentioned by the report, in which husbands and fathers have had their wives and children restored to them through aid of private subscriptions raised in Ireland, “the poor-rates in Ireland not being applicable to such a purpose.”

The newly-enacted law which renders it possible to pension to some extent the superannuated and worn-out medical officer is a step in the right direction, not only as regards the justice of his claim, but also in respect of the interests of the sick poor, and therefore of the community and ratepayers generally.

We observe, however, with regret, that no advance has been made towards recognising the right of the members of the medical staff of the poor-law department to the office of medical inspectors; but, on the contrary, that the special provision, enacted July, 1868, to enable non-professional men to discharge the duty of medical inspectors, has already added to the overwork of the dispensary medical officer by imposing upon him additional work.

That this point requires very thorough ventilation is rendered all the more evident by many circumstances; one that immediately presents itself to us is the growing opposition to vaccination in England, and the difficulty attending its thorough success in Ireland, where, although so much has been achieved, there still exists opposition to it, and in a remote district a leaning towards the "pernicious trade" of one or two hitherto unpunished inoculators.

Steps have been taken, by appointing an additional medical officer in the district referred to, which had been of such extent as to preclude the possibility of that attention necessary to the proper care of the people, and by drawing the attention of the local authorities, by circulars, to the insufficiency of fines inflicted upon those who broke the law hitherto, which latter measure was adopted by the Lord Lieutenant's directions, to render vaccination still more effective throughout the whole island.

How are inspectors who possess no medical knowledge whatever to judge of the success of vaccination? or to hold inquiry into the occurrence of epidemic disease, or the propriety of the treatment pursued in any case in the wide range of practice required of the medical officer?

Altogether, the placing of non-professional men over the heads of medical men who have been called upon to possess special qualifications before they can compete for the Irish poor-law service, is a proceeding manifestly injurious to the best interests of the public service, and therefore of the rate-payers and the poor, and one also tending to keep the best men from entering it. It likewise offers an inducement to such officers to quit the service early, and at a time when their matured judgment and skill render them able attendants upon the sick poor.

The country asks the aid of the poor-law medical officer, as a sanitary officer, at a time of any outbreak of serious epidemic disease. Surely such should be the case at all periods, and the additional time, labour, and skill required by such office should never cease to be properly compensated. To do so is to secure the best interests of the ratepayer and of the poor. For how much have the former to pay and the latter to suffer by the varied forms of chronic and avoidable disease which the poor-law medical officer, if so empowered, could be instrumental to prevent. This we say advisedly, and after extended experience. And it is in this direction, we think, the recommendation of Drs. Stewart, Rumsey, and others, should be made available for the public good. For if every inspector of the poor-law medical department was in future to be selected from those poor-law medical officers who have shown practical acquirements in their profession

and in sanitary science, instead of being selected to meet the exigencies of political parties in the government of the United Kingdom; and if such really able men had under them a band of practitioners extending over the whole country, armed with sanitary powers, and suitably compensated, great indeed would be the benefits arising, not only to the poor and the country generally, but also to science and humanity, from the additional knowledge that might fairly be expected from the labours of such a body of men.

VIII.—Sanitary Supervision of Prostitutes.¹

ENGLISH sentiment has of late, in respect of the subject now about to be discussed, undergone a change, the cause of which it would be interesting to inquire into. Somewhere about twenty years ago a 'Westminster' reviewer found it necessary to enter into an elaborate apology for his boldness in asking the reading public even to notice the great moral sore which was then, as now, corroding and festering in the body politic. Only twelve years ago Mr. Acton, in the preface of his well-known work on prostitution, deplored the persistency with which the public still averted its gaze and virtually ignored the topic. The present state of public sentiment may be estimated by the appearance, in the last July number of the review just alluded to, of an article upon the subject, rushing *in medias res* without a word of apologetic introduction, addressing itself actually to the ladies of England, displaying in glaring colours the physical evils of uncontrolled prostitution; free and unspoken in expression, tracking out for the information of its readers each and every tortuous path by which venereal contamination is believed by the writer to glide into the constitution, and using terms which we confess we have hitherto considered sacred to professional men alone.

We make no comment upon the propriety, the necessity, or the delicacy of the appeal. We simply note it as a phenomenon, and ask what is its import as a sign of the times? We cannot

¹ *Eleventh Report of the Medical Officer of the Privy Council, 1869.—Contagious Diseases Act, 1866 and 1869.—Reports and Papers issued by the Association for promoting the Extension of the Contagious Diseases Act of 1866 to the Civil Population of the United Kingdom.—Report from the Select Committee of the House of Lords on the Contagious Diseases Act, 1866, Session 1867-8.—Report from the Select Committee of the House of Commons on the Contagious Diseases Act, 1866 (communicated from the Commons to the Lords), 1869.*

believe that such an article as this could have been published in a journal intended for all classes of readers, unless there were strong reasons to believe that the public sensibility would no longer be shocked, and that the public mind, masculine and feminine, was prepared for its perusal without a blush. The explanation we conceive to lie in the rapid progress which civilisation has made in matters relating to public health, the ever-widening spread of an interest in these matters, a growing consciousness of individual concern in them, and a more complete appreciation of the connection which exists between physical and moral evils.

Up to about six years ago female prostitution was regarded for all practical purposes only from its moral side, and in its relations to public order and public peace. Where the limits thus imposed were transgressed, the common law of the country stepped in to punish the offender, and special enactments, made from time to time, provided for particular cases methods of special and summary procedure. Solicitation in the public streets was forbidden, disorderly houses and brothels might be suppressed, public-houses were regulated and licensed, refreshment houses and places of public amusement and recreation put under police supervision, and women as well as men without home and apparent means of subsistence were liable to apprehension as rogues and vagabonds.

Philanthropic individuals have devised various plans for the reclamation of fallen women. Here and there penitentiaries have sprung up, supported by voluntary contributions, and no doubt have done some little good. Refuges for women desirous of abandoning their evil life are among the latest developments of philanthropic zeal, and have been filled by the self-denying exertions of charitable people who have devoted their energies to perambulating the thoroughfares and to the holding of midnight meetings, to which prostitutes are invited, and where they are subjected to exhortations to repentance. Far be it from us to discourage those who in any way set aside their own convenience and pleasure with the object of reducing, though only to the extent of a hair's breadth, the seething mass of sin and wretchedness that surrounds us. "There is joy in heaven over one sinner that repents," and it would ill become us to say or to hint that the reclamation of only one solitary woman here and there is a result to be sneered at as incommensurate with the labour and money expended in the task. It is not for us to calculate the value of the result looked at from a religious point of view; but regarding the amount of reclamation effected by this means with a worldly eye, as social reformers, we must concur in Mr. Acton's verdict, that "all amiable as these

schemes be in the abstract, they are, *quoad* the great enterprise, but paltry peddling scratches on the surface of evil."

The year 1864 saw the first attempt of any magnitude to grapple with the evil on its physical side, when Parliament passed the first of the 'Contagious Diseases Prevention Acts.' The first step in legislation was entered upon, not from philanthropic motives, although they may have had some influence, but from considerations regarding the health and efficiency of those defenders of our country whom imperative circumstances require for many years to lead a life of celibacy. Parliament is ever, and very properly, careful not to interfere unnecessarily with the liberty of the dangerous classes of the community; when it errs in this respect, or appears to err, it is always on the side of caution. We cannot blame it for this. Much of our legislation is, and with the temper of our race must be tentative.

It is our practice to construct halting statutes at first and to amend and reconstruct from time to time as the nation accepts their principle, and as their inefficiency is proved by experience. And double the amount of customary caution was necessary in dealing with the subject of prostitution. Probably it would never have been dealt with at all but for the interest which has sprung up as to sanitary matters generally, and that which arose out of the late Lord Herbert's exertions to improve the sanitary condition of our troops in particular. Besides, in avoiding the Scylla of indifference, it was necessary to ensure against being engulfed in the Charybdis of popular opinion. Men had heard a good deal about the French and Belgian system of controlled and registered prostitution; their moral sentiments revolted against anything approaching the legalisation of sin; while not a few of the clergy and many well-meaning religious persons held, almost as a fundamental article of their creed, that to interpose between the fornicator and his natural punishment would be a profane interference with the Divine government, and would throw down one, at least, of the barriers against universal defilement. Still, this serious truth was apparent, that venereal diseases alone entailed upon the nation the loss of the services of the troops stationed in this kingdom alone during an entire week out of each year, or thereabouts. In 1860, 1 in every 4 of the Foot Guards in London suffered thus, wasting the nation's time and money in hospital, and entailing in many instances the entire loss of his services and training by a premature invaliding. Nor was this loss confined to the army. In 1862, of 88,600 men of the royal navy admitted into hospital, 7000 were admitted for venereal diseases; and in 1863, 1482 such cases were admitted into Haslar Hospital alone.

The first Act (that of 1864) came into operation in December

of that year. We shall not pause to detail its provisions, which turned out to be inefficient, chiefly on these two grounds, namely, that it gave to Government no power to build hospitals, and made no provision for the periodical examination of common women. A royal commission was then issued, the end of whose labours was to put matters on a much more satisfactory footing by the passing of the 'Contagious Diseases Act of 1866.' It is to the evidence in our possession as to the working of this Act, and to the encouragement it affords to further advances in the same direction, that we shall devote the remainder of this article, in which we also propose to consider the arguments adduced on either side, by those who advocate and those who are prepared to resist the extension of the Act to the protection of our civil population. The last is a subject which demands very careful and thoughtful treatment. None can judge of the difficulties but those who have made a mental effort to picture to themselves the minute details of police by means of which any efficient scheme having this object in view must be carried out, and have taken some pains to estimate the demands which would have to be made upon the public purse. We do not say that any of these difficulties are insurmountable, or that they forbid all consideration of the sort of extension proposed by the association formed to carry it into effect; but we do say that the topic is not one adapted for such sensational writing as a late reviewer has indulged in, nor for the reckless diffusion of statements as scientific truths which are even now not matter of agreement between earnest and truthful scientific men. Neither is it one to be dismissed as we have been sorry to see it dismissed elsewhere in a few flippant sentences.

The medical officer of the Privy Council thus puts the question as it concerns our military and naval forces :

"It seems to me that prostitution and its attendant diseases, in their relation to the army and navy, are, in two different points of view, matter of public concern and responsibility; first, because the military and naval services at their respective stations are essential determining causes of prostitution, and the State, which for its own purposes keeps those masses of male population unmarried, cannot claim to be indifferent to the result; and secondly, because the specific diseases which arise in that mode of life occasion so enormous a quantity of temporary disablement in the two services as to be of pecuniary importance to the entire taxpaying community."

The case of the soldier or the sailor employed by the State is exceptional and peculiar. A young man in civil life has every inducement to preserve his chastity in the prospect of marriage, which he is free to contract when his means permit him to do so with prudence, and every facility, if he please, to

avoid temptation, to guard his imagination from dangerous pastures in the active pursuit of the business of life, and to satisfy his cravings for female society by the ennobling companionship of the virtuous of the opposite sex.

But it requires no argument to show the very different circumstances in which the units which make up the mass of our soldiers and sailors are placed in these respects. We do not say that fornication is forced upon them, but all ordinary facilities for its avoidance are denied by the inexorable necessities of the two services. They have a moral right, therefore, to some extent, to be protected against its physical dangers, and the State, in its own interests, is politically bound to protect itself from the loss attendant upon its own arrangements.

The chief provisions of the Act of 1866 were the following :—
(1) Twelve military and naval stations were selected in which and within a radius of five miles around which the Act was to operate. (2) The authorities of the Admiralty and War Office respectively were empowered to provide hospital accommodation for diseased women, and also to appoint visiting surgeons, who should examine women brought before them. (3) Power was given to the superintendent of police to bring up before a justice any common prostitute resident in any place to which the Act applies, or if resident within five miles of such place, who shall have been within its limits for the purpose of prostitution; and such justice was empowered to order her to be subject to a periodical examination by the visiting surgeon for a year. (4) Provision was made by which a woman might voluntarily, without being taken before a justice, submit herself to such examination. (5) In the event of a woman being found diseased on examination, the visiting surgeon was directed to detain her, and to place her in a hospital for treatment, where she could be detained in legal custody, either till cured or for a period of six months. (6) The hospital surgeon, on discharging a woman, was to certify whether or not she was free from contagious disease, one copy of the certificate being given to the woman. (7) If discharged uncured, and subsequently found to be acting as a prostitute, she was made liable to punishment. (8) Any woman thinking herself aggrieved by detention, or wishing to be relieved from periodical examination, on account of discontinuing prostitution, might apply to a justice for relief. (9) Penalties could be inflicted upon any owner of a house permitting a prostitute to resort to it for prostitution, if he knew at the time that she was a common prostitute and diseased. (10) There was, finally, the ordinary provision made for enforcing the observance of the Act by fine or imprisonment.

The metropolitan police officers were entrusted with the duty

of carrying out the statute ; the men employed being carefully selected. On the whole, this arrangement appears to have worked well, the men performing a very delicate duty with judgment and good temper. The testimony in their favour has been singularly uniform. It was felt that there would be an advantage in employing men who were free from all local associations and prejudices, and this branch of the service had already a jurisdiction for special military purposes in the places to which the Act applied. In their hands many of the apparent rigours of the Act was mitigated ; and it was found that it was sufficient, for the most part, to arm them with powers, in order to ensure the result desired, without putting those powers into operation. The success of this experiment—for it was an experiment—has been made the subject of two parliamentary inquiries, one by a Select Committee of the House of Lords in 1868, and the other by a Select Committee of the Commons in 1869. The result has been the adoption by Parliament of a number of amendments, which, it is believed, will render the Act more perfect and satisfactory.

In estimating the success which has attended the operations of the Act, we may have regard to three points :—1. The results which have followed as they respect the amount and character of venereal disease among the soldiers and sailors at the stations. 2. As they respect the amount and character of the disease among the prostitutes. 3. Those amendments, in respect to the amount of prostitution and the physical and moral state of the prostitutes, which, although not directly contemplated by the Act, appear to have followed indirectly as a result of its application.

1. *As to the men*, it can scarcely be said that up to the present time the results of the operation of the Act have been quite such as had been anticipated. Although it came into operation in 1866, great difficulties were experienced in several places in bringing it into full force. It is not to be concluded, either, because venereal affections have been less severe in character, that this is altogether due to the Act, since it appears from a variety of evidence that true syphilis had been decreasing, on the whole, among our troops stationed in the country prior to the passing of the Act. Nevertheless, the general results have been encouraging, and especially so in those places where the Act has been applied for the longest time and under the most favorable circumstances. Windsor, for instance, was a station which was one of the worst for venereal diseases to which the Guards are sent ; yet the report of Mr. Wyatt, the surgeon-major of the Coldstream Guards, after a year's operations under the Act, was that this state of things is quite reversed. At Sheerness, where the Act came into operation

very early, namely, in October, 1866, it was stated that in 1868 the disease was almost stamped out, existing only in a nominal shape, with the exception of the men who brought the disease with them. At Devonport, in 1865, in the month of April, with a combined force of 8583 soldiers and sailors, there were 180 men in hospital with venereal disease; whereas, in the corresponding month of 1868, with a force of 10,635 men, there were only 69 in hospital for this cause. In a table furnished to the Committee of the Lords by Mr. Romaine, the progressive improvement among the sailors and marines at Plymouth is shown in an interesting manner by exhibiting the ratio per 1000 of the force in hospital for primary diseases during the different half years from 1864 to 1867. It shows that even the Act of 1864 produced a decided beneficial effect, but that it became much more obvious when the Act of 1866 was applied. The ratio of cases was as follows:

First six months of 1864	(no Contagious Diseases Act).	129·7	per 1000 men.
Second	„ „ (Act of 1864).	120·1	„
First	„ 1865 „ „	104·8	„
Second	„ „ „ „	101·7	„
First	„ 1866 „ „	62·3	„
Second	„ „ (Act of 1866).	60·4	„
First	„ 1867 „ „	49·3	„

Still, the fact that there was a remarkable reduction in the number of cases during the first half of 1866, before the Act of that year came into operation, seems to indicate that the continued diminution which was observed subsequently might have been in part due to some other cause. Altogether, we are disposed to conclude with Dr. Balfour, that although in some places where favorable conditions existed for carrying out the Act, the results were decisive, yet that no absolute and unquestionable inference can be drawn until a few more years have elapsed. The results are, so far as they go, encouraging, and that is about all that can be said about them as concerns the men. One thing, however, all that have observed its operation seem to be agreed upon, namely, that the cases admitted into hospital have been rendered milder in their character, and that the amount of true syphilis has been considerably reduced.

2. *As to disease among the women*, the general experience has been that the amount has been lessened, and its character rendered less severe. Of this there is abundant proof, especially at those stations where sufficient hospital accommodation was provided, and periodical examinations could consequently be carried out. The experience of the four quarters ending March, 1869, as tabulated by Dr. Leonard, shows this conclusively, inasmuch as the number of women brought under examination

at all the stations together was rapidly increased quarter by quarter, while no corresponding increase was noticed in the proportion found diseased; on the contrary, the proportion found free from disease became quarter by quarter larger. In the quarter ending June, 1868, 2542 women were examined, and 1118 were found free from disease; in the next quarter, out of 3532 women examined, 2011 were healthy; in the next, out of 5073 women, 3448 were healthy; while in the last quarter, out of 6974 women examined, 5720 were found not to be diseased. And yet all of these were prostitutes of the commonest class, openly plying their trade. The results obtained in the Plymouth and Devonport district are so remarkable, as showing the reduction in the proportion of cases of syphilis, and in the severity of the attacks, both of syphilis and gonorrhœa, as indicated by the average duration of treatment, that we are justified in presenting them in a table supplied by Mr. Sloggett, the visiting surgeon of the Royal Albert Lock Hospital at Devonport. It shows the condition of the women at three periods; that is, when there was no legal interference, under the operation of the Act of 1864, and, lastly, under that of 1866, as more and more hospital accommodation was provided, and the inspection of prostitutes became more and more complete and comprehensive.

	Total cases of each class.	Syphilis.			Gonorrhœa.		
		No. of cases.	Average days of treatment.	Per cent. of cases.	No. of cases.	Average days of treatment.	Per cent. of cases.
<i>Voluntary System.</i>							
484 days, Dec. 3, 1863, to Mar. 31, 1865 (35 beds) .	282	162	38·78	57·45	120	31·32	42·45
<i>Under Act of 1864.</i>							
548 days, April 1, 1865, to Sept. 30, 1866 (average 58½ beds)	422	196	64·51	46·45	226	50·5	53·55
<i>Under Act of 1866.</i>							
182 days, Oct. 1, 1866, to Mar. 31, 1867 (62 beds) .	155	54	92·11	34·84	101	61·5	65·16
183 days, April 1, 1867, to Sept. 30, 1867 (62 beds) .	168	60	79·21	35·72	108	48·	64·28
182 days, Oct. 1, 1867, to Mar. 31, 1868 (91 beds) .	238	86	74·87	36·13	152	35·84	63·87
183 days, April 1, 1868, to Sept. 30, 1868 (average 136½ beds)	754	193	58·36	25·60	561	22·86	74·40
182 days, Oct. 1, 1868, to Mar. 31, 1869 (162 beds)	835	148	56·84	17·72	687	26·36	82·28
	2854	899	1955

In studying this table, it is to be observed that, until the power of detention was given by the Act of 1866, women could and did leave the hospital uncured, so that the results, as they relate to the duration of illness, can only fairly be judged of during the last of the three periods referred to; and it will be observed, not only how cases of sores have been reduced in proportion to cases of gonorrhœa, but how much the period of cure has been lessened, especially since greater accommodation for the reception of diseased women into hospital has been provided. Similarly good results are stated to have been obtained on other stations, where the Act has been fairly worked. At Sheerness, where venereal disease used to be rampant, only eighteen diseased women were found in the course of the first quarter of 1868, notwithstanding that all the common women in the town were examined. At the first application of the Act at Aldershott the cases of venereal disease were very virulent, and those of constitutional disease numerous. At the present time, however, it is stated that were it not for fresh importations syphilis would be nearly got rid of. In March, 1868, out of 82 admissions, there were only 11 cases of primary syphilis. At the beginning of the same year, notwithstanding forced examinations, the fifty beds provided for diseased prostitutes could not be kept filled. The number of common prostitutes at this station is estimated at about 320, and they are subjected to periodical examination.

3. One result of the operation of the Act has been to improve the physical and moral condition of the poor women in many important respects. It has been found that at Plymouth and Aldershott less annoyance is experienced from solicitations in the streets, and at all the stations the women are becoming more decent, more cleanly, and better behaved. Residence in a hospital where quiet and good discipline prevail, where kind treatment is experienced, and where some religious influences are brought to bear, necessarily tends towards this end; and sooner or later every common prostitute in a district under a system of periodical examination finds her way into the wards, inasmuch as every one sooner or later becomes diseased. Few women, too, are so far unsexed by their mode of life as not to have left some grain of self-respect, which is capable of asserting itself on occasion. When a woman finds that she must present herself to the surgeon at intervals for examination, it has been observed that she usually takes some pains to make herself presentable, so far as cleanliness is concerned; and when a woman has once been sent into hospital, and has there acquired habits of cleanliness, she does not usually abandon those habits on leaving it, although she may immediately return to her old life. And the acquisition of these habits benefits the woman and the

district which she frequents, since they render her less liable to become diseased, and preserve her from the chance of suffering from those frightful forms of sore which are engendered by the complication of syphilis with filth and wretchedness. Mr. Sloggett's belief is that in Devonport and Plymouth the number of prostitutes has actually been greatly reduced, partly from the moral influence brought to bear upon them while under treatment, and partly because a great number of women get ashamed of constantly being known as prostitutes. "In 1864 [he says] there were 2000 known public prostitutes in Devonport and Plymouth; there are now 770. Those numbers which I give are not vague." Of course the smaller number remaining are gainers by the decrease, as may be seen from their being better dressed than they used to be.

It may be supposed, and with some probability, that many of the women who thus disappeared from the district merely migrated to some other spot where they would be less disturbed. If they did, their places do not seem to have been filled by other women. But Mr. Sloggett has reason to believe that a large proportion were led to abandon prostitution altogether. He says—

"Since October, 1866, when this Act first came into operation, I have made nearly 9000 examinations in 1775 individual women; that is, the same women being examined over and over again. There now remains in this district only 770. Of the remaining number, which will be in round numbers 1000, upwards of 300 (but I will say 300) have certainly abandoned prostitution. . . . Those women for two and a half years have left the streets, and are no longer on the streets. They are gaining honest livelihoods. In addition to that, 250 are married, and leading, in the belief of the police, virtuous lives. That will make 550 who have really given up prostitution."

Another proof of the good done in this way is to be found in the tables published in the report of the Select Committee of the Commons. From one of these it appears that in the course of the year ending March, 1869, 4864 women were admitted at all the stations into hospital, and of this number 391, or about 1 in every 13, either entered a house or was returned to her friends. It has more than once happened that the police have thus been agents in taking young and unhardened prostitutes from the streets in an early period of their career. We have no hesitation in saying that the reformatory results of the operation of this Act throw entirely into the shade those which have been hitherto obtained by the well-meaning attempts of philanthropic people, who have approached the women with direct religious and moral advice, while they were

in the full pursuit of their avocation, and incessantly indulging in drink and dissipation.

When it is considered that the very essence of the Act of 1866 was a forcible interference with the liberty of a class of women who, beyond all others, have thrown off the ordinary trammels of restraint, whose existence is the embodiment of wilfulness and license, it is interesting to take note of the readiness with which common prostitutes have been found to submit to its provisions. Much of this facility in working has been attributed to the judicious manner in which the Act has been executed by the Metropolitan Police. Perhaps an equal share is due to the gradual manner in which it has been necessarily introduced at the several stations, on account of the insufficient hospital accommodation. The women had time to become acquainted with it, and accustomed to the interference of the police. At first it was practicable only to examine and detain women who had actually been discovered to have infected men taken into the military or naval hospitals, for it is clear that without beds it would have been fruitless to examine prostitutes indiscriminately. A woman thus taken and detained, cured and discharged, would naturally spread such a report of the treatment she received as would render others less fearful of detention. Again, it was found that the women who were first taken told upon others whom they knew to be diseased, "Why do you take me," would one of them say, "when So and So is as bad as I am; why don't you take her?" Their rough notion of justice inclined them to submit the more readily on condition of all being treated alike.

Another thing has assisted the police very much, namely, that the women generally had sufficient shame about them to desire to avoid the publicity of an inquiry before a magistrate. Hence, on being warned to go to the visiting surgeon for examination, the greater number by far went voluntarily without compulsion, availing themselves of the 17th section of the Act. During the year ending March, 1869, 17,128 women submitted voluntarily, against 33 whom it was found necessary to take before a magistrate. The chief difficulty then was that if a woman subsequently failed to present herself periodically, the law could not compel her. She had to be warned again. This is a state of things, however, which the amended Act of 1869 has set right. Still, it is a fact, that among the women the Act, though calculated to benefit them immensely, is not popular. Some, as before remarked, evade it by leaving the districts where the Act is in operation, and continue to carry on their trade. The women, however diseased, and knowing themselves to be so, have no compunction in receiving and infecting several men

in a night, so long as they can do it without too much pain and inconvenience to themselves. They only give in spontaneously when they are so ill that they can go on no longer; and then instances have been known of women travelling from distant towns in order to get received into one of the authorised Lock Hospitals. At some stations this has become so common a thing as to be notorious, and has played its part in keeping up the more severe class of venereal diseases in districts where otherwise it would almost have been crushed out. The isolation of Sheerness has rendered it less liable than any other station to this cause of disturbance, and apparently, as one consequence of this, the suppression of venereal diseases here has been more complete than at other stations.

But there are two parties to the spread of venereal diseases. Not only do the women of a district infect the men whom it is the object of the Act to preserve effective and healthy, but the men infect the women, and thus mediate one another. Indeed, one witness asserted that a woman receiving, as some do, a large number of men in succession at one time, might herself escape, while the men might in this way distribute disease from one to another. It has been found, as a matter of fact, that the introduction of a new battalion into a town or station has resulted very shortly in an increased number of admissions, not only into the Lock Hospital, but also into the military hospital. And the same thing has followed both here and abroad, as at Malta, where a strict regulation of prostitutes has been enforced for some time, on the arrival into port of a ship whose crew has immediately had liberty on shore. Dr. Balfour meets this fact, which he does not deny, by the explanation that newly arrived "men get among a set of women that they know nothing whatever about, and among them are a certain number of what we used to designate 'fire-ships;' the men, before they know what women are diseased, manage to contract disease and come into hospital." Possibly this may be one reason, and another is that when an influx of fresh soldiers comes into a station, a certain number of fresh women follow them, and these women are frequently badly diseased. Still, admitting these explanations, it is difficult to see how the men who have mingled with these women should not have been more diseased than those to whom they are added at the stations where the women are more or less looked after.

Dr. Balfour rests his explanation upon the efficiency of military discipline, which requires that every man should report himself to the surgeon as soon as he finds that he is diseased. He believes that this is actually done, that the men do thus report themselves, and that they are induced to do so by the

fear of punishment; but there is a good deal of evidence the other way, which seems to show that the tenpence per day which men have to pay while in hospital actually serves to debar them from reporting themselves, and that they brave the chances of discovery. Indeed, it is in evidence that, on the examination of one regiment marched into Devonport, under a general order from the Commander-in-Chief, twelve cases of disease of the most virulent description were discovered. Surgeon-Major Wyatt tells us that his experience in the Coldstreams, where a periodical examination of the men has long been in force, is, that it is a common thing for men to try to conceal their disease. "I find," he says, "that the men report themselves only on the morning of the inspection, and they do so then because, if detected with disease at the inspection, they are punished. The man has evaded reporting himself to the last possible moment, although he may have had the disease from the last inspection." There is credible evidence, also, that certain chemists at Aldershot have driven a roaring trade in supplying medicine to soldiers who have preferred paying their charges to going into the military hospital. We infer, therefore, that there is a danger in military stations of the extension of disease from newly arrived soldiers, who bring it with them, although it may be impossible exactly to apportion the actual increase of cases observed under such circumstances between this cause and that assigned by Dr. Balfour.

We believe Dr. Balfour was the only witness examined before the two committees, who did not give prominence to the influence of a fresh influx of new soldiers or sailors, and of latent disease among soldiers, in increasing the amount of disease by actually newly infecting the women. And hence has arisen the idea that the periodical examination of soldiers was an important and necessary complement to the periodical examination of the women. We have already alluded to the fact that the Commander-in-Chief has already authorised an examination of the men on removal from one station to another. But the periodical examination of men at intervals of ten days or a fortnight is altogether a different question. That such an examination, systematically and effectually made, is practicable; that, conducted with every precaution for the maintenance of decency and secrecy, it is not openly objected to by the men; and that it has had excellent results, is testified by the evidence of Surgeon-Major Wyatt, of the Coldstreams. This testimony disposes of one or more of the objections raised against it by Dr. Balfour. No doubt the medical officers dislike the duty—it certainly cannot be an agreeable one; but as a military duty, and carried

out as such, it is not to be regarded as degrading in its character.

But even were such periodical examinations as have been suggested carried out, the benefit would not extend appreciably beyond such purely military stations as Aldershott, and Windsor. There would still be a similar danger at the maritime stations arising out of men paid off from ships or allowed leave on shore. In the case of ships' crews who have come off a long voyage, and have been long confined to their vessels, and who have had the benefit of surgical supervision during that period, the danger is obviously less than that from merchant seamen from foreign ports, who notoriously bring home with them and spread among the women the most severe forms of syphilitic disease. How is this influence to be guarded against? It has been proposed to subject all of them by law to a medical inspection before they are permitted to come ashore, or, in the case of merchant seamen, before they are allowed to receive their wages. We very much doubt the practicability of this arrangement. It is a proposal which trenches upon the next question we shall touch upon, namely, *the extension of protection against venereal diseases to the civil population of the country.*

Unfortunately this topic, which, if it be deserving of discussion at all, ought to be considered with sobriety, as involving interests of national importance, has by some writers been dismissed hastily with a few epigrammatic sentences, which have not, and perhaps were never intended to convince, while in other quarters it has given occasion for unseemly and intemperate contention. Yet it is one which has been thought worthy of earnest consideration by a good many serious men, who in various walks of life have seen and deplored the extent of a great evil, and have given the sanction of their names to a project for its abatement.

The extension of the Contagious Diseases Act to the civil population is a proposal which must be viewed on more than one side; it has a political, a social, a moral, and a financial aspect. It will conduce to perspicuity if we marshall the arguments in favour of or against it, as they range themselves under these divisions.

1. *The political aspect of the question.*—The association which has been organized to promote the extension of the Act of 1866 to the civil population “holds that sufferers under any kind of contagious disease are dangerous members of society, and should, so long as they are in this state, be prevented from communicating it to others.” In order that there may be no mistake in the political argument it puts forward, we have quoted the words of one of its reports, where the objects of the association are

set forth. From this, as a major proposition, the line of argument is clear enough. Venereal diseases are contagious; persons suffering from them are dangerous members of society, and so long as they continue diseased they should be prevented by the state from communicating their disease to others. This is no doubt a very easy and plausible mode of settling the whole matter; but somehow, when we are presented with a syllogism, we are apt to suspect that it is used to cover defects which would be more apparent were the question put in a different style. If we allow that it is the duty of the state to protect its members under all circumstances from persons who are likely to endanger them, whether by communicating diseases to them or in any other way, then of course we must admit that politically it is the duty of the state to prevent persons with syphilis or gonorrhœa, with itch, or porrigo, from giving these maladies to other people. But are we prepared to allow this? Even confining the question to venereal diseases, are we prepared to allow the interference of the state? Can we bring ourselves even to imagine the state interfering in the case of every individual, man, woman and child, to prevent the extension of these diseases to others?

We are writing in serious soberness, for we wish to show that there is a fault in the proposition which the association starts from. It places no limits, no conditions upon the duty of the state in preventing contagious diseases. Let us try and put this straight, by asking what is the duty of the state in respect of protecting its population by laws from physical dangers? We think that two principles may be laid down upon the basis of which this question may be answered; for it must be obvious to any one that a population of Englishmen is not to be treated by the state as children who are entirely unable to take care of themselves. In the majority of cases it is sufficient, and has been practically found sufficient, that the common law provides compensation to a sufferer who, without his own reckless concurrence, has undergone physical damage at the hands or by the culpable negligence of his neighbour. The first principle of British legislation may be thus expressed:—That the state will only interfere where there exists a considerable danger to a large section of the public, against which the exercise of ordinary care, prudence, and foresight will not suffice for protection. We may illustrate this by instancing the provisions made by law for the safety of travellers by railway or by ships, for the disinfection of public conveyances, and generally for the abatement of nuisances established upon premises over which the sufferer has no control. Now, it certainly cannot be said that the exercise of ordinary care and prudence will not protect a man from

venereal disease. If he puts his head into the lion's mouth, he is very properly left to take the natural consequences of his folly ; and the fornicator has the less right to ask for protection because his conduct is immoral, although not legally criminal ; that public and private immorality is a state of things against which good government ever sets its face ; and, finally, that it stands in the way of marriage, which public policy can alone countenance.

The second principle which we would enunciate is one which rather qualifies the first than stands alone as a principle of legislation ; it is this : that even where a considerable danger exists to a large section of the population, against which it cannot protect itself, legislation is slow to interfere where the dangers apprehended arise out of those natural laws of Divine government which affix trouble, or even death, as the penalty for their transgression. That these penalties extend beyond the first transgressor to those closely associated with him, although it be a thing to be deplored, is nevertheless not a circumstance which will warrant the exceptional intervention of the state. The sins and follies of a father are visited upon his family, his children, and his connections ; commercial imprudence throws a family into poverty ; the drunkenness of the head of a family, or his conviction of crime, not only leads to his own early incapacity or death, but involves the disgrace or ruin of all who are connected with or dependent upon him ; and syphilis places its brand of shame upon wife and offspring. Yet the state will not prevent a man from bringing pecuniary ruin upon himself and those about him, from disgracing them with himself, or from bequeathing to his children an inheritance of disease. We say the state is slow to interfere, not that it will never interfere ; the line which limits its action is ill defined, and it is well and necessary that it should be so. One thing we consider the state is bound to do, namely, to take care that there shall be no public exhibition of immorality, and that the prudence of individuals shall have full play by withdrawing solicitation to sin from the public streets.

2. *The social aspect of the question.*—Mr. Simon puts this question, “ Does the detriment which venereal affections cause to the public health reach those limits at which principles generally preferable ought to be exceptionally abandoned by the state ? ” To quote the florid language of the ‘ Westminster Review ’—

“ Thousands upon thousands, chiefly of the lower classes, but partly of the higher, are the innocent and defenceless victims of a pestilence whose march is so secret, and whose attacks are so insidious, that none can be certain of escape ; many a trusting maiden

radiant with happiness, health, and beauty, who gives herself in marriage, speedily finds her joy turned to mourning, her health to disease, and it may be her beauty defaced by its loathsome poison; many a mother has to deplore the contamination, not only of her own constitution, but that of her child, to which, either before or after birth, in countless instances, that poison proves fatal."

"Thousands upon thousands," "many," "countless numbers!" These are days of accurate statistics, so let us ask, how many? What is the extent of the public damage? What is the present extent of such venereal disease, "which is undermining the strength and indirectly destroying the lives of a large proportion of the adult male population," and producing all the sad mischief just described. The association, in the same report that we have before quoted from, speaks of "the venereal disease" "as a contagious disease of the gravest character, which is constantly transmitted from parent to offspring," and refers to a report of the Harveian Society, issued in 1867, on the prevalence of "venereal disease." We may, therefore, assume that all the writers we have just mentioned who subscribe to the statement issued by the association mean by "venereal disease" that which the Harveian Society means by that term. Well, then, let it be understood that in the statistics issued in the report of that society are included all diseases directly the result of an impure connexion—gonorrhœa, simple venereal sores, and true syphilis.

The argument in favour of state interference, based simply on the extent of "venereal diseases," as included in the report of the Harveian Society, or in similar statistics, is consequently *ad captandum vulgus*. Of all these forms of disease there is only one which extends its influence decidedly beyond the person actually and primarily in fault. If a man contracts a gonorrhœa, it is true he may damage his wife or his child, but the outside of the permanent injury to himself is urethral stricture and its consequences; and if he suffers thus no one particularly pities him. Much the same may be said of the man who gets a simple sore; he may be mutilated, and if he be it serves him right. This is the popular verdict, and we are not disposed to quarrel with it. Practically it is only true syphilis which ought to be regarded in relation to the present question.

To what extent, then, does syphilis prevail among our population, as a disease undermining strength and destroying life? First of all, we may say that it is not a disease which is fatal to adults in its primary or secondary manifestations, and, moreover, that perhaps the majority of those who have undergone the disease thus far, live as long as they could otherwise have been expected to live, and die of diseases with which

syphilis has no more to do than the man in the moon. They are active, useful, and wealth-producing members of society so long as they continue in it.

“At the present day especially,” says Lancereaux¹ (than whom a higher authority could scarcely be quoted), “when syphilis still inspires exaggerated fears, it should be known that this disease becomes dissipated completely, in a great number of cases, after the cessation of the cutaneous eruption, and perhaps sometimes with the primary lesion.”

Next we may say that it is not true, as asserted by the association, that venereal disease, even limiting the term to true syphilis, “is constantly transmitted from parents to offspring.” We wonder very much at such an assertion appearing in the report when we read the names of some of those connected with its management. Again we quote Lancereaux :

“An infected father does not necessarily transmit the disease from which he is suffering. More than this: we see children born and grow up healthy, both of whose parents are, or have been syphilitic.”

The highest chance of transmission to offspring by either father or mother occurs during the primary and secondary stages of the disease, a period which is not commonly prolonged beyond two or three years from the first infection—usually not so long. Time and treatment modify subsequent transmissibility.

No doubt of late years our acquaintance with the later results of prolonged constitutional syphilis has been vastly extended, and we have come to recognise organic conditions as syphilitic, which formerly would have been differently grouped. Yet can it be affirmed that any very large proportion of adult deaths or disabilities is distinctly and unquestionably due to those deep-seated manifestations of chronic syphilis? It is true there may be a reason why the death registers at Somerset House fail to throw any light upon this subject, but, we ask, what other statistical proof is afforded by those who dwell in argument upon this character of the disease? Again, it is true that the course of some disorders is unfavorably influenced by the syphilitic taint, that syphilis may evoke the manifestations of scrofulous or of a tubercular diathesis, but it by no means is to be accepted, as proved at present, that it can engender these diseases either in the individual or his offspring, or that it is, by any period of incubation or any modification of hereditariness, capable of transmutation into them. The amount of nonsense

¹ Syd. Soc. Edit., vol. ii, p. 120.

written upon this and allied subjects, in the sensational article we have referred to so often, is most deplorable. To those who believe in such transmutations we would commend, for their peace of mind, the perusal of Lancereaux's remarks "on the influence of syphilis on other diseases." They may probably find some public compensation made by syphilis in the opinion expressed by some authors, that syphilis is actually a preservative against the operation of some morbid causes, and so against some other diseases. We must call upon them to investigate the question and to strike the balance of public damage, before they invoke so doubtful an averment in aid of their appeal for legislative interference.

But recourse has been had to hospital statistics. How can hospital statistics assist us to discover the amount of mischief effected by syphilis on our population? What we want to discover is the proportion of our population suffering directly or indirectly from this cause. At the best, hospital statistics can only tell us of the proportion of sick persons whose illness is attributable to this cause—and, indeed, not even this accurately, for it can only tell us of a certain class of our population more liable than any other thus to suffer, and of these, only of such as apply to hospitals for relief. But no satisfactory statistics even of this character have yet been put forward by the promoters of the extension of the Act of 1866 to the civil population. They announce and make the most of the announcement that, from inquiries which have been instituted, from one third to one fifth of the surgical out-patients of our general hospitals in London suffer from venereal disease, but then they do not tell what proportion these bear to all the out-patients applying, nor do they limit their figures, as we hold they ought to be limited, to true syphilis. And as regards the Lock Hospital in London, it is, of course, very shocking to be told that 36 per cent. of the female out-patients are respectable married women, but this does not help us out of our difficulty. We are at a loss also as to the assistance our inquiry is to receive from the records of ophthalmic hospitals. No one who is acquainted with syphilis is ignorant that the eye is one of the most common seats elected by that disease for its constitutional manifestation, or would express or feel any wonder at the proportion of syphilitic cases which a special hospital of this kind admits.

Even if the statistics afforded were unquestionable, we should be no nearer the solution of the question we want answered; and as respects the middle and upper classes, who consult their private medical advisers when in trouble, or more commonly some specialist unknown to their families, we despair of any information at all. But apart from the fault of

grouping all venereal diseases together, as proper matters for state intervention, grave doubt has been thrown upon the statistics, such as they are, which the association parades. The medical officer of the Privy Council has taken some pains to test their accuracy. At the Children's Hospital, in Great Ormond Street, he finds, on inquiry, that of 118,590 children of the poor treated there during the last ten years for all sorts of diseases, the proportion recorded to have been syphilitic has only been $1\frac{1}{2}$ per cent. From a very careful investigation conducted by Mr. W. W. Wagstaffe, who visited for this purpose several of the largest general hospitals and dispensaries in London, it appears that, of 9363 out-patients in all departments of these charitable institutions, only 8.71 per cent. were affected with venereal diseases of any kind, and only 4.21 with syphilis, the remainder being mostly cases of gonorrhœa.

Mr. Wagstaffe extended his inquiry to the in-patients treated in hospitals, workhouse infirmaries, and by the parochial out-of-door surgeons, with the result "that 6.92 will probably represent the per-centage of the sick poor population affected with some form of venereal disease," of which about half, or $3\frac{1}{2}$ per cent., would be infecting syphilis. Surely 455 persons suffering from true syphilis in one form or another, in a poor population of a million and a half, such as that which seeks gratuitous medical aid in our London population, cannot be held to be a proportion so large as to call for exceptional action on the part of any Government. Mr. Simon adds, "It must be remembered that London, probably, illustrates the utmost dimensions which the evil can attain in this country." It is only fair, however, to remark that this estimate is a minimum; it takes no account of that class of poor persons whom a desire for concealment induces to consult and pay small fees to practitioners of a regular or irregular stamp, persons who for other ailments would probably have recourse to a hospital or dispensary. Still, we question whether this addition would very materially affect the general inference.

3. *The moral aspect of the question.*—Is it right and conducive to good morals that any deterrent motive should be withdrawn from those who are tempted or disposed to indulge in promiscuous fornication? This is a question which relates to the men rather than to the women, who, we presume, are little likely to trouble their heads about the matter. They do not mostly enter upon their course of life merely for the indulgence of lust. Taking a male population, all classes together, we very much doubt whether, where brute passion is concerned, such possible consequences as the onset of disease interfere to any considerable extent in withholding men from giving the rein to their desires. They would not select a companion whom they knew or believed

to be diseased, but otherwise, if we mistake not, they shut their eyes, and take their chance of escape. The motive suggested has small influence, except among a minority. Still there are those who hold the contrary opinion, and maintain that, under any circumstances, the safeguards of chastity ought not to be removed. Judging from the list of bishops, deans, archdeacons, and clergy who have become members of the Association for the Extension of the Act, we must conclude that the idea of danger to male chastity by lessening contagious disease in our prostitutes is in course of being given up. As to the women, we must say that experience is in favour of the proposed movement. In the face of the evidence adduced before the select committee of the two Houses of Parliament we cannot regard the amount of prostitution as a "fixed quantity," even regarding the amount of male requirement as unalterable. Mr. Simon says:—"I apprehend that the concubinage market, like other markets, tends to be fed according to demand," and we quite agree with him; but then it is quite consistent, even with the law of supply and demand, that prostitution generally should be lessened by the recourse of the same number of men to a smaller number of women. And we have produced some evidence that this result has followed the introduction of the Act at certain of our military and naval stations: and in the case of those women who have continued their trade, it is in evidence that they have been cleaner in their persons and habits, better behaved, less disturbing to the public peace, and less demonstrative in the public streets. All this has been an unmixed good, and as such we should all desire to see it extended throughout the country.

4. *The financial aspect of the question.*—Supposing that it were considered desirable, on political, social, and moral grounds, to extend the Act of 1866, or rather, we should say, the amended Act of 1869 to the civil population, what would it cost, and how could the expense be met? Of course there must be examining surgeons in the towns and places where prostitutes chiefly congregate, a large number of specially selected police officers, hospitals or lock-wards with their staff, and the diseased prostitutes detained must be fed and often clothed. And all this would cost a great deal of money, which must either be paid out of the state treasury, or be provided by local taxation. We will pass over the difficulty of providing the hospital accommodation, which would be no trifling one, if we may judge from the objection raised in some places to the reception of lock cases into general hospitals, and also over the difficulty of accommodating the vast number of diseased seamen, who, if syphilis is to be suppressed in our ports, must also be looked after in some way. To come to the money question. How many beds would be

required for the diseased women in England, and what would they cost? It is calculated that in London alone there are, or were five years ago (and the number is not likely to be less now), 6000 common prostitutes, known to the police, carrying on their trade in the streets. The population, being taken in round numbers at three millions, and assuming the demand to be on the average pretty much the same throughout the country, we may infer that there are not likely to be less than 40,000 common prostitutes in England and Wales, of whom we may consider it probable that at least one third, or 13,300, are at any one time the subjects of contagious disease. This, then, is the smallest number for which hospital accommodation would have to be provided; the smallest number, for there are plenty of diseased women also amongst those prostitutes who carry on their trade in a more respectable manner than those whom we are now speaking of. The annual cost of each bed, with the attendant hospital expenses, could not be calculated at much less than £40, which, without including the expenses of the police, and the first cost of hospital buildings, would entail upon the country an annual tax of somewhere about £522,000, or over half a million per annum. Probably it would not be fair to say that the British ratepayer would be the poorer by the whole of this money, for at the present time he has to contribute pretty heavily to the support of diseased women, pregnant prostitutes, and their offspring occasionally, in and out of the workhouse, which is now their only refuge when ill; and of this he would be relieved. At present he pays the money without exactly knowing all that he is paying for; but separate the diseased prostitutes, and tax him for their support and cure, as a separate item of charge, and he will be certain to raise the time-honoured growl, and demand why he, a steady family man, should pay in order that his neighbour may be defended from the disorders which he earns. If the idea of defraying the expense by local taxation be abandoned on this ground, is it more likely that any Government would dare to propose its being defrayed by a Parliamentary grant? If money is to pass from the public treasury for sanitary purposes, it may be fairly argued that other sanitary objects have prior and higher claims. Let them be satisfied first.

But, apart from the expenses of extending the Act, which probably after a few years' working might be something reduced, the financier would ask what chance there is of his getting his money's worth. The thing he is asked to pay for is the almost annihilation of syphilis by an elaborate system of medical examination of prostitutes, soldiers, and sailors. Is it likely that syphilis would be so far reduced in amount among the innocent

of our population as to make the system practically a success? We fear not. It would scarcely be possible to bring the Act to bear sufficiently broadly among the women who infect men; and, with the exception of soldiers and sailors, the men who infect women, or mediate one another, would never be reached at all. For it must never be forgotten that there are grades and ranks of fallen women. Probably not more than half the women who prostitute themselves in London are known to the police, or are likely to be known, except by the adoption of a system of inquiry such as would be utterly repugnant to our English customs. There are probably thousands of girls in London who are actually following some industrial employment, but who, nevertheless, either to add something to their miserable wages, or to provide themselves with finery, or from the love of gaiety, practise prostitution, and who, though not to be ranked with common prostitutes, are in the direct path of becoming such. Others are never seen soliciting in the streets. They are above it, a better and an upper class, well lodged, well fed, well clothed, and well paid. How is this mass of clandestine prostitution to be reached? Yet, these comparatively decent and attractive women are among the most active agents in the dissemination of syphilis. Clandestine prostitution is the rock upon which foreign systems of regulation have broken up, and it will prove fatal to any measure here, the object of which may be the annihilation of syphilis among our civil population.

On the whole, we may say that we do not think the time has yet arrived at which the proposal to extend the operation of the Contagious Diseases Acts to the civil population can be properly entertained. These acts are yet upon their trial. Deficiencies of various kinds were found in the Act of 1866, and these have been remedied. It was found, for instance, that the extent of the districts under supervision was too narrow; they have been made wider. It was found desirable to enlarge the schedule of districts by including some neighbouring towns; to abandon the practice of giving certificates to women discharged cured from the hospitals, and to give the surgeon the power of relieving women from periodical examination, thus facilitating their return to an honest mode of life. It appears to us that it would be wise to await the result of these amendments; to let the experiment go on for some years longer under these more favorable conditions, and even then, should the course of events prove satisfactory, that it would be better first to enlarge the schedule of districts so as to include every military and naval station of importance in the United Kingdom. Were it not that we regard these acts as tentative, we should be disposed to press for an early extension of this nature. If our

land and naval forces are properly objects of care and protection against venereal diseases, it would appear incomprehensible that the protection should be afforded at one station and not at all stations. We are not certain that we might not go further and say that, were all the military and naval stations thus scheduled, civilians would be as fully benefited as they are likely to be by such an extension of the operation of the acts as is contemplated by the well-meaning association whose schemes we have been considering.

IX.—Phthisis in Italy.¹

THIS essay was written in answer to the question proposed by the Venetian Institute of Sciences on the causes of the diffusion and increase of phthisis pulmonalis in the chief cities of Italy, and on the best means of counteracting them. After a brief introduction the author produces the statistical information he has been able to collect from Venice, Treviso, Verona, Mantua, Milan, Turin, and Genoa. He compares the deaths by phthisis in these cities with the general mortality, and also with the population, and avails himself of other sources of information to determine the question. He brings under review allied morbid conditions, such as scrofula, rickets, &c., and makes use of the tables of conscription for evidence of physical imperfection, &c. He tests the value of statistical data, and shows the corrections to be made in them. He studies phthisis in the larger and smaller cities, and in the suburbs and open country; its fatality in the kingdom generally; the possible errors of diagnosis, its distribution in the two sexes, and at various ages, and in different employments and trades as well as in the army and penitentiaries; its relation to the civil condition, circumstances, means of subsistence and to season of the year. After employing the necessary corrections, he concludes that in Upper Italy the mortality from phthisis pulmonalis is nearly 27·6 in 10,000 inhabitants, and causes a thirteenth part of the deaths. Dividing into two groups the eight principal cities of this portion of Italy, he finds that more die of phthisis proportionately in the larger cities, though their general mortality is less than in those of smaller size.

¹ *Intorno alla diffusione della Tisichezza polmonare, alle sue cagioni e ai provvedimenti piu valevoli di combatterla. Ricerche del Cav. Prof. ALFONSO CORRADI, M.D., &c.*

On the Spread of Pulmonary Phthisis, its Causes, and the best means of staying it. By Prof. ALFONSO CORRADI. Prize Essay. Venice, 1867. Pp. 156.

In his second part the author contrasts, for certain cities, the old recorded data with the new, but finding here little certainty he has recourse to indirect evidence, the silence or brief mention of phthisis in early literature and in historical records, compared with its modern predominance and the increase in this our day of disorders which, in his opinion, bear some affinity to phthisis, such as cancer.

He next proposes the question, if pulmonary phthisis is not now apparently more prevalent owing to our attention being more drawn to it; whether there may not be displacement of one malady by another, and what share vaccination or syphilis, contagion or hereditary disposition, may play in its evolution. The fourth part of his work is practical in the sense of hygiene. He insists on the absence of a special cause and the folly of seeking a special remedy. *Fortes creantur fortibus et bonis* is the motto of his essay. He looks much to a radical or inherent defect or weakness of constitution derived from progenitors as a cause, and ends by expressing a wish that, in the forthcoming congress to be held at Florence, some surer basis may be established for future statistical returns, as at present such returns are too often unreliable, and most difficult of interpretation. Beginning with Venice and other cities of the Venetian territory, he finds that within a term of four years, from 1862 to 1865, there died of pulmonary phthisis in Venice 1953 persons, that is, 488 yearly; the entire number of deaths amounting to 15,975, and the mean yearly mortality to 3988, exclusive of soldiers and still born (which last are put out of computation with regard to all these cities). Here, then, the deaths from phthisis would amount yearly to 122·3 in 1000 deaths from all causes, or 40·1 calculated on 10,000 inhabitants, with a population in Venice of 120,506 inhabitants. Compared with the other cities these numbers are the highest of all, and whether and to what extent they admit of diminution we shall presently see.

In Verona during the five years, 1853-7, the mortality from phthisis was 577 (annual mean 115), and the total mortality 10,693 (mean 2138); so that in every 1000 deaths phthisis was the cause in 54, or 22 from that cause of death in 10,000 living, with a population of 52,292 souls. In Padua the proportions are higher, viz. 68·9 in 1000 dead, and 32·2 reckoned in 10,000 living; the population about 35,000. The total of deaths one year with another being 1611, and those from phthisis 111, reckoned on the four years 1862-1865. Mantua seems more fortunate; it counts 28·5 among 1000 deaths, or 44 in 10,000 living, in a population of 30,000; a mean yearly mortality of 1495, of which 42 by phthisis. These calculations are rather old, being derived from the statistics of the three years 1836-38.

Treviso shows a worse state of health. On a mean mortality of 513 there are 44 from phthisis yearly; 81.4 per 1000, more than eight per cent. of the deaths. The population being nearly 16,000, there are 27 deaths from phthisis for every 10,000 living. Pulmonary inflammations, cachexia, and tabes senilis have each, as in Treviso, a separate column.

Leaving the Venetian provinces, we find in Milan the sum of deaths from phthisis, during the three years 1863-5, to be 2299 or 766 yearly; the total mortality during this period 27,796 (mean 9265). Thus of every 1000 deaths 83.4 from phthisis, and 31 calculated on 10,000 living inhabitants. The population of Milan and of the Corpi Santi in the census 1862 amounted to 245,000. From this we must subtract something for the hospitals, country folk, casual visitors, and foreigners. For it is to be noted that of 2299 deaths from phthisis 203 were country people. Thus city statistics are swelled and those of the country lightened. Similarly in Turin, from 1862-1864, three years inclusive, the deaths not strictly appertaining to the city were 1803; the mortality of this and of its country districts (*contado*) being 20,649 or 6883 yearly. In these more than twenty thousand deaths, 1620 occurred from phthisis, that is 543 yearly. Therefore we may reckon 78.4 per 1000 deaths, and with an incidence of 26.3 in 10,000 inhabitants, the census of 1862 giving 205,712 inhabitants for the Commune di Torino.

Only one year's statistics, that of 1860, are available for Genoa. Deaths from phthisis 318, that is 74.7 per 1000, and 216.5 in 10,000 inhabitants. In Sassari, in the island of Sardinia, with 23,985 souls, in the two years 1860-61, deaths 1585, of which 78 from phthisis. The calculation here is 49.2 in 1000 deaths, and 16.2 in 10,000 living, or, subtracting strangers, 44.7 and 14.2. These returns we have copied textually from the essay, to which we must refer for their grounds of authenticity. Neither Florence, Rome, Naples, or Palermo furnish a satisfactory basis for medical statistics. As concerns Naples, Drs. Achille Spatuzzi, and Luigi Somma, who have paid great attention to the mortuary returns in that city, affirm that a sixth or seventh of the whole mortality is due to pulmonary phthisis. and another sixth or eighth to scrofula and rachitis; also Dr. Enrico di Renzi, in a competition essay on the alimentation of the lower classes in Naples, 1863, laments, along with the writers just named, that there should be always among children such a number of deaths from tuberculosis without a true certification, for they are either returned as from catarrh or some abdominal complaint, and their tubercular nature, the pathological element, overlooked. With respect to tuberculosis in adults, he marvels

greatly that the city of Naples is fully as much liable to phthisis as either London or Paris, though the salutary condition of the climate should render it far less common. If his estimate be correct, it is the cause of one eighth of the deaths, and, consequently, surpasses that of Turin or Milan, where it is about one twelfth. As to the notices of Oesterlen, Boudin, and Pietra Santa, they are wholly unreliable. The Cav. Salvatore di Renzi has for subject of a *memoire*, "Whether the climate of Naples is favorable or not to the production of phthisis" ('Filiatre Sebezio,' 1839), in which he attacks the groundless assertion of Journé, that in Naples one patient out of 2·34 is phthisical; at the same time he admits the deaths from phthisis to be 1 in 12 of the total mortality. Our author then refers to the observation of Dr. Jacquot, in his "Lettres sur l'Italie" ('Gaz. Méd. de Paris,' 1853, p. 532), that in Civita Vecchia there are many phthisical patients, but fewer, he thinks, than in France. In reference to Rome, no positive facts exist, and in Tuscany there are no statistics, if we except those of the excellent Salvagnoli, regarding the marshy district of the Maremma. According to him, from A.D. 1840 to the year 1846, out of 15,000 sick in the province of Grosseto only 227 had tubercular phthisis, which is 1·5 in 1000, or only ·5 in 1000 permanent inhabitants, these amounting, in all, to 462,000. The population, however, is very sparse, 231 inhabitants to 1000 hectares of land. Ask the faculty in Florence and other Tuscan cities if they are acquainted with tuberculosis; they will answer that both the pulmonary and abdominal forms are very frequent indeed, and that they encumber the hospitals.

Vivenot, in his book, 'Palermo und seine Bedeutung als Clima-tienter Curort,' Erlangen, 1860, would convince us that in Palermo tuberculosis is not prevalent. His proofs are wholly inadequate, being derived from the hospital of Olivuzza, which receives only a scanty number of phthisical inmates. He does not take into account the prejudice Palermitans entertain against hospitals, on account of the contagion they believe to be the foundation of phthisis. He offers no statistics. On the other hand, Professor Maggiorani declares tuberculosis to be common in Palermo, not very frequent, indeed, under the form of acute meningitis in children, or of pericarditis and peritonitis in young people, but very common as pulmonary, laryngeal, and intestinal phthisis. According to Professor Bosi,¹ Ferrara exhibits the highest mortality from tuberculosis. Moreover, Professor Sinalgalli has observed that in the pathological school of Pavia, in the decennium 1855-64, out of 2352 subjects furnished by the

¹ 'Lezioni di Medicina Pratica.' Ferrara, 1859, p. 566.

civic hospital of that city, and examined there, in 202 certainly, or say one eighth in all, he found tuberculosis in some stage. A small proportion of these were of the earliest period of life. Still more, by an old rule of the hospital, chronic cases, and hence the phthisical, as well as children, except in infancy, are excluded from the use of the hospital, so that the computation does not comprise a fair average of tubercular cases. If we take the before-named nine cities, of which we possess statistics, and rank them according to their mortality from phthisis, we shall find, on the scale of 1000 deaths from all sorts of diseases, that they fall into the following diminishing series:—Venice 122·3, Treviso 86·4, Milan 83·4, Turin, 78·4, Genoa 74·7, Padua 68·9, Verona 58·5, Ferrara 49·3, Mantua 28·4. On the other hand, if we compare the number of deaths from phthisis in the proportion of 10,000 of the population, the series will be as follows:—Venice 40·4, Padua 32·7, Milan 31·4, Treviso 27·3, Genoa 26·5, Turin 26·3, Verona 21·9, Sassari 16·2, Mantua 14·1. The last series may be held to be the more valuable and instructive. On comparing the two we see that five cities occupy the same place in each, Venice being always at the head; the last three corresponding in both series, and Genoa being always in the middle; Padua, Treviso, and Turin vary in proportion in the two series.

From the meteorological and medical reports of Venice, by Drs. Namias and Berti, for the year 1862, it appears that, among 488 deaths attributed to phthisis, some other pulmonary affections are included. The author of the present treatise has arrived at the conclusion that a twenty-eighth part of all the deaths should be deducted from such diseases; even then the deaths from phthisis in Venice on 1000 deaths and 10,000 living will be respectively 87·6 and 28·6, instead of 122·3 and 40·4, before quoted. Perhaps for Treviso and Padua, on the score of imperfect diagnosis, some such deduction may be made. In Turin we find enumerated more deaths from chronic pulmonary affections than are found in Milan, and this may be a reason why Turin may appear to have fewer deaths from phthisis than that city. In fact, if we may believe the statistics of Dr. Rizzetti, more than half the class of chronic pulmonary complaints in Turin is formed by chronic broncho-pneumonia and chronic bronchitis, which constitute 3 per cent. of the mortality. Only in Turin and Sassari are the chronic pulmonary affections particularised in the returns. In those of Padua and Treviso the chronic pulmonary inflammations are not distinguished from the acute or from other disorders of the respiratory apparatus. In the Verona tables they are allocated with chronic affections in general, and no mention is made of them in Padua. Still, if we

take our reckoning from the statistics in which they are mentioned, we find them, with respect to the total of the deaths, to have a mean of 4 per 100, and of $51\frac{1}{2}$ per cent. when grouped and compared with those of phthisis. The increase of mortality in the larger cities from foreigners and country people, non-residents, is subject to much variation. In Milan the country people alone form an eighth of the mortality. Only in Genoa and Sassari is the fact of native origin indicated in the tables. For these cities they might be estimated somewhere about 30 per cent.; but that would hardly suffice for Milan. The number of strangers who die in Milan is excessive compared to Turin and Padua; but even such a deduction as we have named would reduce the mortality from phthisis in Milan from 766 to 536 yearly—*i. e.* from 83.4 to 74.4 in 1000 deaths; from 31.1 to 21.8 in 10,000 living. Again, when we consider the reputation which Venice enjoys for relief of phthisical affections, we must allow an important deduction in the returns, on account of sick strangers who come there. In Sassari and Genoa we are not left to conjecture. The deduction that is found for strangers in the first named will alter the return from 49.2 to 41.7 in 1000 deaths, and from 16.2 to 14.2 in 10,000 living. In Genoa the figures become reduced from 74.7 to 64.9, and from 26.5 to 18.5.

We now proceed to show that phthisis and acute complaints of the chest do not spread *pari passu*, nor acknowledge the same causation. For acute inflammation of the chest, Italian cities lie in the following decreasing order:—Verona, 104.7 in 1000 deaths, and 42.8 in 10,000 inhabitants; Genoa, 101.3, and 36.8; Milan, 81.0, and 30.4; Turin, 78.5, and 25.8; Venice, 78.2, and 26.2; Sassari, 65.6, and 21.6. Here we have two cities heading the list, which, in relation to phthisis, were respectively in the seventh and fifth places. According to Dr. Castelli, in Verona pneumonia produced in the course of sixteen years 1588 deaths, and phthisis, *with vomica*, 1760 deaths. The mean annual proportion would be 99 and 110. Taking the mean of chronic and acute chest affections as regards 10,000 inhabitants, the cities above named range thus in the following decreasing series:—Padua, Verona, Genoa, Turin, Milan, Treviso, Venice, and Sassari; but when we calculate the death rate from phthisis in the scale of 1000 deaths, Genoa takes precedence of Verona, Milan succeeds to Treviso and Venice. After effecting these corrections, it would still result that in all the preceding cities (Sassari placed out of the reckoning) situated in Upper Italy—*i. e.* the Venetian states, Lombardy, Piedmont, and Liguria, pulmonary phthisis attains to the figure of 27.6 deaths in every 10,000 inhabitants, and to a thirteenth

part of the mortality, or 75·6 per 1000. According to Oesterlen, an annual mortality of 2·5 in 1000 inhabitants, and a quota of one ninth (11 per 100) in the deaths may be esteemed normal, or pretty favorable. On one count then Italy is above, on the other below the average. If we exclude Mantua, which may very well be done, on the score of the statistics being old, the proportion of phthisis in Upper Italy would reach 28 and 78 respectively; but the incidence, as it would appear, is unequal, there is more of the disease in Lombardy, and less of it in Piedmont and Liguria. The smaller cities exhibit the greatest general mortality, but they have fewer deaths from phthisis; they show on this comparison about the same amount of difference in their returns of phthisis pulmonalis that the Belgian statistics displayed between town and country, viz. 41 and 36, when an inquiry was made into this subject from 1856-59.

Incipient phthisis often passes for bronchitis, and even in dissections of the tuberculous the local lesion may be found repaired; or, again, patients may die of intercurrent diseases. In some hospitals, as that of Milan, we find a table for hæmoptysis, and another one for tabes; 466 inscribed in the first, 4304 in the latter. In Venice marasmus heads a list, and there are other dangerous pitfalls. There is yet another difficulty to be surmounted. Are we to follow the teaching of the French? Is phthisis essentially tuberculosis, or not rather, in many instances, agreeably to the school of Tübingen, a chronic catarrhal pneumonia? Tuberculous phthisis, in ordinary form of speech, is made to include both forms. According to the expression of Hedingen, "Und Tuberculose ist noch immer der für beide Zustände gültige Ausdruck."¹ No other malady like chronic bronchitis and pneumonia appears to bring on tuberculosis. The author agrees with Hedingen, that in many ways catarrhal chronic pneumonia may be looked upon as scrofulous pneumonia ('Hedingen,' p. 63). It is on this account that Corradi has given so much importance to the subject of scrofula and personal development, to muscular tenuity and width of chest, as tested by the spirometer, and to the tabular views from the army returns, especially those of conscription; yet he fails to arrive at any precise result, beyond the terrible predominance of the disease in all the armies of Italy. He gives, however,—without vouching for its accuracy, an extract from the journal 'Igea,' No. 20, 1867, from the hand of Professor Lombroso. Upon 4221 deaths in the Italian army in the year 1864, there died of phthisis 679, more than 16 per 100 deaths. In 272,750 men

¹ Hedingen, Aug., 'Die Entwicklung der Lehre von der Lungenschwindsucht, &c.' Tübingen, 1864.

there were 1099 tuberculous, a somewhat less proportion than in the French army. As to the season of year, from statistics of six cities of Upper Italy, he finds the worst season is summer, the least fatal, autumn, and between the two, but nearer to the first, stands winter; spring, as regards phthisis, is the most even, or nearest the mean. Turin has more deaths in winter from this terrible scourge, Treviso in spring; Verona, Padua, Treviso, are more spared in the winter time, Turin and Milan in autumn. Nevertheless, the influence of season on the mortality from phthisis is often not considerable, very much less so indeed than upon the general mortality. If we look at the table below we shall see that for Venice and Verona, July; for Milan and Turin, the month of March; for Padua and Treviso, May, supplies the largest tale of death. Respecting the month of smallest fatality, there is no fixity; it is, so to speak, different for each city. Nor does phthisis keep even pace with pneumonia. In

Months arranged according to the Mortality by Phthisis progressively decreasing.

	Venice.	Milan.	Turin.	Verona.	Padua.	Treviso.
I	July	March	March	July	May	May
II	August	July	May	December	June	April
III	November	October	January	June	August	December
IV	December	September	July	April	December	March
V	March	April	August	January	July	July
VI	October	December	June	August	March	June
VII	January	June	February	November	November	August
VIII	February	November	September	March	January	November
IX	September	May	April	October	September	October
X	April	August	October	September	February	January
XI	May	February	December	May	October	September
XII	June	January	November	February	April	February

Verona, for example, during the five years from 1853 to 1857, the order is quite contrary. As regards mortality from phthisis, summer, spring, autumn, winter, occur in a decreasing scale; for pneumonia, in a reverse or an increasing order. The same in fact as holds good for apoplexy, cerebral congestion, and sudden deaths, which depend on the state of the air and meteorological conditions. Phthisis owns no such dependence. Dr. Castelli, who was occupied for many years with medical statistics in Verona, has observed,—

“Phthisis, unlike pleuropneumonia, is not subjected to atmospheric conditions; far from so, in its annual incidence we find something invariable and constant. Thus, in the year 1855, when

cholera was bad in Verona, 120 persons died with phthisis, 120 and 102 in the two preceding, 119 and 116 in the two following years."

The question whether phthisis is a modern disease, growing apace with our civilisation, has received no striking elucidation, as far as we can see, in these pages.

In regard to the antagonism of ague and phthisis, it is borne out by no other fact in Italy than that of the Province of Grosseto; it is belied by other facts.

The author has an interesting note on the stonemasons who have to work at the granite near the *Lago di Garda*, who are said nearly all of them to die towards fifty years of age. The same is true regarding those who work the slate in the caverns near Chiavari, who suffer from the *Mal de Chiapperolo*, as it is called. Those who do the same work in the open air are but little subject to the disorder, neither is the dust found in their lungs.

Of the general causes of phthisis, those which would seem to be most operative in Naples are certainly bad sustenance and wretched habitations, more fit for the lower animals than for man. Only the *facchini*, or porters, among the lower classes, have a proper share of food. Much the same causes affect the health of Venice; want of work, defect of cleanliness, and poverty.

Professor Marino Turchino¹ has impressed upon the Government the necessity of providing a better class of dwellings for the lowest of the poor, by which means their mental no less than their bodily health might be improved. We shall not be tempted by the idea of making this review more complete to borrow from our author further hints or further head of argument. In his conclusion he returns to his original idea, which he amplifies very skilfully:—" *Fortes creantur fortibus et bonis.*"

¹ 'Sulla Igiene pubblica della città di Napoli.' Napoli, 1862, p. 57.

X.—Lunatic Asylum Construction and Organization.²

THIS copious and instructive Report owes its origin to a resolution of the Government of New South Wales, taken in anticipation of a contemplated re-organization of the asylums of the colony, and of a proposed revision of the laws regulating the condition and supervision of the insane. Before committing themselves to the special legislation intended, and to the con-

² *Report on Lunatic Asylums.* By F. NORTON MANNING, M.D. By authority. Sydney, 1868. Pp. 287.

structional proclivities or fancies of asylum architects, they most wisely sought to inform themselves respecting the existing plans and arrangements for accommodating and treating the insane, pursued in the principal countries of the civilised world. For this object they selected Dr. Manning, a physician in whom they had full confidence from his experience in the management of the insane; a confidence justified by their personal knowledge of his fitness for the special duty entrusted to him at the time of his appointment to it, and now amply attested by the published results of his mission.

Dr. Manning was instructed to "visit the chief asylums in the United Kingdom, on the Continent, and in the United States," and to collect plans, statistical and dietary tables, and whatever details relating to the construction, management, and discipline he could gather, that might serve in "a re-organization of the lunatic asylums of the colony, on the basis of a correct knowledge of the improvements carried out under more favorable circumstances in other parts of the world." The instructions received were faithfully fulfilled, and we have in the volume before us a mass of particulars respecting the construction and management of asylums, that will render it a most valuable work of reference to all concerned in these matters. However, Dr. Manning regrets a skilled architect was not associated with him, in order to judge of those details in construction, which only such a professional man could rightly be expected to understand. Nevertheless, there is, in respect of this omission, this consolation, that Dr. Manning was not expected to become architect of an asylum, and that the numerous plans he has appended to his Report will enable architects to judge sufficiently accurately of the details of construction followed in Europe, to arrive at an opinion of what is necessary and useful in building a colonial institution for the insane.

In collating the materials collected during his tour of inspection, Dr. Manning has treated of the several asylums according to the general principles of construction and organization exemplified in them, and has supplemented this account "by a series of suggestions for the establishment of an asylum system, . . . with particular reference to local and other conditions existing in New South Wales." Moreover, as he travelled to discover only what was worthy of consideration and of imitation, he has laid aside the character of critic, "and passed lightly over special faults dependent on management and not on structure, except where such faults may illustrate a point in the Report, or afford evidence of value against a system of management."

Following out the plan laid down, the author has first noted the division of the insane which must determine the character of the accommodation to be provided, viz. into pauper, non-pauper and criminal insane, idiots and inebriates. For the first-named class, the pauper or indigent, provision has been made in private dwellings, farm asylums, and close asylums. Their distribution in private dwellings is a plan pursued in every country, more or less ; but, leaving the peculiar colony of Gheel for the moment out of the question, in no country has it been so methodised and turned to good account as in Scotland. But, although giving full credit to Dr. Mitchell's representations of the advantages of the system, and to the praiseworthy zeal of the Scotch Commissioners in supervising its application, Dr. Manning is not insensible to its evils, and remarks on the number of pregnancies occurring in imbecile or insane patients lodged with cottagers, and on the sometimes undesirable life the patients lead among unsympathising neighbours. His conclusion is, that a certain proportion of the chronic insane may, in almost all countries, be so boarded out ; not so, however, in newly settled ones, such as New South Wales, where the fitting accommodation is wanting, "the population a changing one, the mass comparatively wealthy, and the class, with whom lodging and care is found for the patients in Scotland, does not exist" (p. 9).

Gheel, as a matter of course, attracted the author's attention, and a very good sketch of the colony is the result. The impressions derived from his visit are that the patients found there—

"Are more happy, and, in many respects, as comfortable as they would be if provided for in a close asylum, with its many appliances, and its artificial life ; as well washed, as well bedded, perhaps as well fed, they are not ; they have not the constant medical care, and the varied amusements of an asylum ; but they are leading a life which is to them a natural one. There is a freedom, spite of camisole and leglet, which cannot be found in ordinary asylums ; and the chances of cure are as great, if not more so. It must always be remembered that Gheel is intended, under present regulation, for the more quiet and chronic cases only."

Hence the statistics of the place must not be compared with those of ordinary asylums. "But it cannot be doubted by all who carefully examine the subject, that the colony is good in principle, useful to the country and to the insane, and, in most respects, excellently managed. . . . To make another Gheel is impossible" (p. 14).

The farm-asylums which have of late years sprung up in France have not received the attention they deserve, and we are glad therefore to find that Dr. Manning made a point of visiting

the first and principal establishment of the sort, commenced in connection with the Asylum of Clermont as a private enterprise, by the three brothers Labitte. The original asylum itself is, considered as a private institution, of unusual magnitude, containing as many as 1000 patients, but it "differs otherwise little from ordinary proprietary asylums both in England and France." The distinctive features of the establishment are the two colonies (situated the one at two, the other at four miles from the town asylum), to which all the quiet and manageable patients are drafted, and where labour is made the principle of treatment, and all the conditions of life assimilated as nearly as possible to those of common existence. The special characteristics of the whole institution are,—

"The large amount of land under cultivation; the freedom from restraint in the shape of walls, &c., or '*l'air libre*,' as Dr. Labitte himself calls it, and the care with which one part of the establishment is made to work with the other, so that the whole shall be as much as possible self-contained and self-supporting. The entire establishment is said to be extremely remunerative, whilst the price at which indigent patients are received is less than the sum which they are found to cost in most of the departmental asylums of France" (p. 18).

The next class of asylums the author passes under review are those to which the name of lunatic asylum is especially attached, and which, to distinguish them from the collection of insane in lodgings, colonies, or farm institutions, French writers have called "*Asiles Fermés*," close asylums. The section devoted to these institutions for the insane is consequently the most extensive in the book. It contains the author's reports on the plans followed in respect to the site, the aspect and the distance from a town of asylums; on the land allotted them, the separation of acute from chronic cases, the size of asylums, and the details of structure, and arrangement of the wards and supplementary buildings; and on the organization and the mode of management both of the institutions and of their inmates. In short, he presents a summary of his observations of the structural peculiarities and of the internal economy of the principal establishments for the insane which he visited in the course of his tour in Europe and America.

It is worth while to cull from this summary a few memoranda. The treatment of the insane poor in workhouse wards is regarded by Dr. Manning as a great mistake. In England, and still more so in Scotland, much has been done to lessen the evils of the detention of insane in such receptacles, but it would seem that, in some parts at least of the United States of America, the condition of the lunatic wards of the workhouses, and of their

unhappy inmates, is most deplorable. Dr. Manning quotes from Dr. Willard's Report on this subject to the Legislature of the State of New York, an account so repugnant to the commonest dictates of humanity as might be deemed incredible, had the information been derived from any other source. To his own general remarks on the said state of poor-house wards in the United States at large, Dr. Manning says,—

“The main cause impeding improvement in these institutions is the system of government in which the ‘guardian’ element, and this not altogether free from political spirit, is very apparent. The medical officer is not the superintendent of the institution; there is a divided authority, a vicious system in the appointment of servants, and, with all this, a parsimony, which is happily a rare thing in the noble institutions for the insane in the United States” (p. 20).

The divergence of opinion in England and in North America relative to the best method of warming asylums is remarkable.

“With the exception of the Bristol asylum (writes Dr. Manning), open fires, either alone or in combination with some other means, are used in all the English asylums mentioned above; and those asylums in which they exist, either alone or with the addition of air-chambers, the warming is in every way satisfactory. The hot-air system, which is extremely expensive, is by no means always a success, and may be pronounced a decided failure as regards the Bristol asylum. Hot water seems a useful mode of warming for the rooms devoted to the sick, who are liable to feel changes of temperature with especial acuteness, and for whom, therefore, a more fixed and general warmth is necessary; and in the department for the violent class, where open fires are likely to lead to mischief, and their cheerful appearance is not appreciated by the patients” (p. 46).

On the contrary, the superintendents of North American asylums have laid it down as a rule, that such institutions are to be heated by passing abundance of fresh air over pipes or plates containing steam, not exceeding 212° temperature, and placed in the basement of the building. The boiler for generating steam is to be placed in a detached structure, and the air from without is to be driven over the heated plates and through the shafts constructed to convey it into the several wards and rooms. Dr. Manning is inclined to view this system of warming favorably, regard being had to the severity of the climate in North America in winter. It, however, appears to us that this reason is not sufficient to counterbalance the disadvantages of the plan, as exhibited in imperfect ventilation, unnaturally dry air, and general stuffiness. A combination of the open fireplaces with heated flues suggests itself as preferable where the former prove insufficient.

The author is not favorably impressed with the results of schools in asylums. From his own observation, and from information collected, little or no actual educational work is carried out in them; and he quotes the opinions of superintendents, to the effect that they are useless or only valuable as a pastime. As a pastime—a means of collecting together, with a professed object, a number of patients who would otherwise lounge about the wards, mope, and, it may be, get into some mischief, we should, nevertheless, regard the school as a valuable adjunct to an asylum; and we believe, further, that a school might attain even more important ends, and serve as an agency of no mean power in the many-sided system we call “moral treatment.” But, judging from our own knowledge, which tallies with what Dr. Manning reports, the asylum schools are not worked in a manner that would command success. They are committed to the hands of incompetent persons, or of *employés* charged with other duties sufficiently absorbing, or are irregularly carried on, or otherwise conducted on some stereotyped notions imported from ordinary schools outdoors, inapplicable to the place and the people concerned. “The American lecture-system, wonderful in its organization, and eminently characteristic of the people, has found its way into the asylums, and takes a prominent position among the ‘amusements’ therein,” says Dr. Manning, and this fact indicates clearly enough that the minds of the insane can be enlisted to take interest in instruction, if only they are rightly approached, and the agency used be appropriate.

Dr. Manning passes under review the system of reports and statistics pursued in the asylums he visited, and has done well to print in the appendix selections of the forms in use in British asylums for presenting at a glance the daily history for the information of the superintendents. The matter of the statistics of insanity is not largely discussed, for he very properly accepts the conclusions of those who have more especially made such statistics a study, that the data for arriving at accurate results are still wanting. As to the reputed causes of insanity, we are glad to have a statement from him of those that are special and peculiar in New South Wales, and which are thus set forth:

“A. A large number of the early population came under exceptional circumstances, and the relations of crime and insanity are very intimate. The number of insane in a criminal population is always large. In New South Wales in 1846, according to statistics prepared by Dr. Campbell, there was 1 insane in 450 of the bond, and 1 in 1780 of the free population—a vast and striking difference.
B. The changes of early colonial life—its ups and downs—and the

mental tension which accompany the excitement of soon-got wealth, and the depression caused by its frequent and rapid loss, cannot but be largely productive of insanity. Under this head may be reckoned the influences of the gold-diggings in this and neighbouring colonies. c. The lonely life of the shepherd, alternating with long periods of debauchery. d. The abuse of ardent spirits in a warm climate.

“On the other hand, some causes potent in the production of insanity exist here in much less force than in other countries. The grinding poverty, the struggle for existence, taxing both mind and body, and so acting as a predisposing and exciting cause of insanity, is here almost unknown; whilst a ‘pre-eminent salubrity’ of climate, and out-door habits of life, are strongly calculated to diminish the amount of mental disorder. The proportion of insane to population has undoubtedly increased during the last twenty years” (p. 111).

This increase Dr. Manning attributes to the growth of a large mass of chronic insanity, the maximum of which he imagines must surely be reached. The proportion of insane to population is estimated at 1 in 387.

In examining the question of the curability of insanity, the duty of securing early treatment is insisted upon, and, at the same time, the example of some American states is held up for imitation. In those states a premium is awarded to the friends of patients for their early remitting them to the public asylums. This is done—

“By enactments, which direct that when a person in indigent circumstances, not a pauper, becomes insane, and satisfactory proof is adduced that his estate is insufficient to support him and his family; or, if he has no family, he shall be admitted into the asylum, and supported at the expense of the freeholders of his county, for one, two, or three years, without being placed on the list of paupers. Under this law, all artisans, small farmers, and other persons of small estate, seek at once a refuge in the asylum, where they are supported free of expense, and their property remains for the benefit of their families; instead of waiting, hoping for a cure, and trusting to treatment outside the asylum walls, whilst their estate is being gradually dissipated, and they are brought nearer and nearer, and at last into, pauperism” (p. 115).

Necessarily, Dr. Manning reports upon mechanical restraint and seclusion as practised or abolished in the several countries he visited. From his account it appears that the Americans are the most determined opponents of non-restraint, which is all but universal in England, and is rapidly gaining ground in France and Germany. Respecting the opposition of the American physicians, he makes this very pertinent observation, that in all the arguments used by them in favour of restraint, “the evils of seclusion are pointed out, but there is, as a rule, more seclusion, notwithstanding the restraint, in American than in

English asylums." Moreover, he disclaims the assertion of the greater violence of American patients as a myth, stating distinctly that they are not more violent than the patients in English asylums. Again, touching the much insisted on fact that non-restraint and seclusion are in direct proportion, he adduces his experience in Continental asylums, according to which it appears that "wherever the amount of restraint was found to be larger than ordinary, the amount of seclusion was large also" (p. 121).

A good account is given of the principal State Criminal and of the Idiot Asylums; but leaving this we pass on to what may be considered the more important division of his book—the chapter of suggestions—reflecting, as these do, the impressions made on the writer's mind by the extensive survey of asylums permitted to him, and the lessons which he has derived from it. To some extent his conclusions on many points are scattered up and down in the previous portion of his Report, but in his "suggestions" such stray inferences are gathered together and employed in deciding on a course of action to be taken with reference to the provision for the insane in the important colony which he has made his home.

The Scotch and Belgian systems of boarding insane with cottiers he rejects as inapplicable to New South Wales, at least for many years. But he desires to educate the people respecting the general traits of insanity, and the mode of dealing with it, by familiarising them with the working of asylums, and securing their interest therein, and by encouraging them to receive back into their families, relatives, or friends, in whom insanity has become chronic, and for whom asylum treatment provides no further efficacy. Such cases to be placed under medical supervision, and reported upon to a central authority every three months. He moreover advocates a mixed system, consisting of a "close asylum," conjoined with outlying farm colonies and detached cottages.

Several pages are occupied with an analysis of evidence, taken at different times, respecting the actual provision for the insane made in the colony of New South Wales, and the requirements for the future; but, passing by this, we will continue to notice the author's suggestions. He gives preference to proximity to a large town as the site for an asylum, and estimates for one acre of land to every two patients, being double the minimum quantity approved by the English Commissioners. Although he recognises the advantages of erecting separate buildings for acute and chronic cases, under certain conditions of population and of locality, yet regard being had to the present condition of the colony, he recommends two

separate institutions for acute and chronic patients together. Further, with respect to the size of asylums, he considers that 400 to 500 is a preferable number, and that 600 should never be exceeded. Where 500 are to be accommodated, he would make six divisions, and employ the pavilion or block plan, supplemented by cottages; or, otherwise, would blend together the house, pavilion, and cottage plans, as is done in the new Surrey asylum.

He accepts the "dry-earth system" for closets, as preferable to water-closets; open fire-places for warming both wards and rooms, and natural ventilation by opposite windows, supplemented by inlets and outlets in connection with shafts, and with "chambers around the fire-places." The day and night accommodation for patients he would make separate and distinct. He approves the plan of a common dining-hall, but would have one for each sex, and recommends single rooms to be provided for not more than one tenth or one twelfth of the inmates, the remainder being lodged at night in dormitories containing not less than six nor more than twenty beds.

In the matter of the government and organization of asylums, Dr. Manning expresses himself in favour of the system of France and Belgium, where centralisation is more in force than in Great Britain. For his own colony he recommends a Board of Control, appointed by the Government, "for each asylum or for each district," consisting of not less than five members, one or two of whom should be medical men. He would entrust to this Board the control of the finances, the appointment and dismissal of all officers, the visitation of the asylum, and the inspection of the forms of admission, and also the ordering of the discharges of patients. Besides these local Boards, he likewise proposes a general Board of Inspection for the whole colony.

In the asylum itself, however, he approves of an autocracy in the person of the medical superintendent, whom he would pay adequately, in order to secure the services of a man of education, and of the highest professional rank. He would farther allow the superintendent to engage in consulting practice in lunacy cases, as a measure calculated to benefit the public rather than the individual, inasmuch as the former would thereby be enabled to obtain the advice and assistance of a physician specially versed in such cases; an advantage otherwise obtainable only by those chiefly who might reside near large cities in which special lunacy physicians are practising.

With respect to the question of the extent of the medical staff necessary, Dr. Manning says that "in an asylum containing acute and chronic cases in the usual proportion," there

should be one medical officer to every 250 patients. He is decidedly adverse to the appointment of consulting medical officers.

As to the classification of patients, he proposes "that at least six divisions should be made in every asylum for 500 patients;" but he is opposed to founding it—

"Upon the durability of the insanity, its curability, or incurability, or in what has been called the industrial system. The only proper classification is a medical one, and if to this is added a consideration of the social condition, and the peculiar psychological individuality of the patient, everything necessary in the way of classification will be attained. The sick and infirm—all those aged and worn out, and suffering from extreme dementia, as well as the worst cases of epilepsy, for whom special provision, in the shape of furniture, bedding, nursing, is necessary; the cases recently admitted, as requiring special medical treatment, by reason of their mental condition; and the more noisy and violent class—those less under self-control, and sometimes needing temporary isolation—are the only patients for whom classification can be recommended. For the whole remaining body of the patients, a psychological arrangement rather than a classification should be adopted" (p. 208).

Idiots are not referred to, because Dr. Manning very properly looks upon them as unfit inmates of a lunatic asylum, and as requiring a special institution. Convalescents he would place in detached cottages; epileptics he would not group together in the same wards, nor detach the industrious, working patients from the rest, in distinct communities; for every patient should be more or less a worker, and the separation of the willing workers as a class to themselves, involves the loss of example, and the appearance of labour on compulsion. Moreover, it is frequently to the advantage of those who are associated together in the same industrial pursuit during the day, to mix at other times with those otherwise engaged.

We shall not follow the author in his remarks on criminal lunatics and idiots, but bring the notice of this very instructive work to a close, by directing attention to the copious appendix, replete with details of asylum work, as shadowed forth in the examples of reports, of tables, and of programmes of entertainments, collected from various asylums of the United Kingdom. Another important and most valuable appendix occurs, viz. of plans of many of the best modern asylums of Great Britain and the Continent; an appendix that will be highly appreciated by those interested in the erection of asylums—a class of individuals, we may add, represented by every asylum superintendent—for every such officer, sooner or later, affects to be an architect, and to have most decided views as to what asylum

construction should be. As a parting word, we must thank Dr. Manning for his most able and instructive Report on Lunatic Asylums.

XI.—Theory of Contagion.¹

It would be rash to say that any one department of the science of medicine has been cultivated recently with more zeal or success than another; but, if we were to apply the test used by Themistocles, we should probably find that, while each of us rated most highly the advances made in his own favorite branch of our art, common consent would assign the second place to the extension of our knowledge of contagious diseases.

Probably, as Griesinger suggests in the volume before us, much of this progress is due to the great epidemics of cholera, which have done good service by overturning the old hypotheses on contagion, and by calling attention to those sanitary laws which men violate at such a terrible cost.

The attack made by Broussais upon the medical system current in his day had a very similar effect, not by its own teaching, but by the counter-revolution which led Louis and Bretonneau to a more careful study of the symptoms of the acute specific diseases, and so to a better idea of their nature.

¹ 1. *Germinal Matter and the Contact Theory*. An Essay on the Morbid Poisons. By J. MORRIS, M.D. Second Edition. London, 1867. Pp. 111.

2. *Notes on Epidemics, for the Use of the Public*. By F. E. ANSTIE, M.D. London, 1866. Pp. 179.

3. *Traité des Maladies Infectieuses*. Par W. GRIESINGER, Professeur à la Faculté de Médecine de l'Université de Berlin; Traduit par le Dr. G. LEMATTRE. Paris, 1868.

A Treatise on Infectious Diseases. By W. GRIESINGER, Professor in the Faculty of Medicine in the University of Berlin; Translated into French by Dr. G. LEMATTRE. Paris. Pp. 556.

4. *On Famine-fever, and some of the other Cognate Forms of Typhus*. A Lecture. By R. VIRCHOW, Professor in the University of Berlin. London, 1868. Pp. 48.

5. *Études sur les Maladies Éteintes et les Maladies Nouvelles*. Par C. ANGLADA, Professeur de Pathologie Médicale à l'Université de Montpellier. Paris, 1869.

On Extinct and New Diseases. By C. ANGLADA, Professor of Medical Pathology in the University of Montpellier. Paris. Pp. 646.

6. *On Epidemics, Studied by Means of Statistics of Disease*. Read before the Public Medicine Section of the British Medical Association, 1868, by A. RANSOME, A.M., M.B. Manchester. Pp. 18.

7. *Disinfectants and Disinfection*. By R. ANGUS SMITH, Ph.D., F.R.S. Edinburgh, 1869. Pp. 136.

8. *The Disinfectant Question*. A Review of Dr. Angus Smith's Book, Reprinted from the 'Sanitary Record.' London, 1869. Pp. 31.

The industry of British observers, scattered as they are over the whole world, has perhaps contributed most largely to the pathology of this class of diseases; but we may further boast of the strong impulse given to the study of epidemics by Jenner's wonderful discovery.

In spite, however, of the many positive results obtained by medical observation, chemistry, and microscopy in the last few years, considerable obscurity still surrounds the whole question of the nature, origin, and prevention of contagion; and the present moment seems, therefore, favorable for examining what has been already done, and for pointing out in what directions future inquiry is likely to be successful.

If we confine ourselves merely to the hypotheses as to the nature of contagion which now prevail, the following appear to be the chief:—

Dr. Morris, in the little work before us, suggests that the propagation of contagious diseases is only an instance of the independent vitality of what Dr. Beale has called "germinal matter," and others, "embryonic cells." He teaches that this substance has the power of multiplication when deposited in living tissues, and that its simple transference will therefore convey disease from one person to another. A very disproportionately-developed part of the book is intended to prove that this germinal matter may be conveyed by the air, and that "contagio distans" may thus be accounted for.

This would probably be no difficulty to any one who accepted the rest of Dr. Morris's theory; the real objections are rather—first, that we have reason to believe that embryonic cells, when not constantly supplied with fresh nutriment, rapidly lose their vitality, and degenerate into ordinary pus-cells; and secondly, that no sufficient reason is given why some diseases are highly contagious, in which the germinal matter thrown off cannot be very abundant, while others (characterised by a considerable production of this substance) are strictly incommunicable from one person to another; for instance, should not impetigo or eczema be, on this hypothesis, more contagious than measles or scarlatina? Finally, the mere multiplication of embryonic cells, and their death by conversion into pus, is no explanation at all of the differences in *kind* between contagious diseases.

A second theory, which has been especially put forward by Dr. Richardson in England¹ and by M. Robin in France,² professes to account for contagion by what chemists call "catalysis."

¹ 'Social Science Review,' *passim*, and lecture 'On the Poisons of the Spreading Diseases.'

² 'Mém. de la Soc. de Biologie,' 1863, p. 95, and 'Gaz. Médicale,' Fév., 1864.

The contagious principle—which in the case of pyæmia Dr. Richardson tells us is an alkaloid—is supposed to act catalytically on the albuminous matter it meets with, reproducing itself, and finally poisoning the whole organism; very much in the same manner as diastase has the power, while itself apparently unchanged, of converting starch into grape-sugar by its mere presence.

We shall soon consider the medical bearings of this theory; but we may make two remarks on it here from a purely chemical point of view. First, it should be remembered that modern chemistry tends to show that in all these cases of apparently mere action of contact the *catalyzer* (to use a convenient word) is not itself unchanged. For instance, we used to be taught that, in the ordinary process for obtaining hydrogen, the sulphuric acid acted catalytically by its mere presence; we now know that the hydrogen is derived from the decomposition of the very substance which was before supposed to be inactive. Or again, Sir Benjamin Brodie has shown that, in the decomposition of binoxide of hydrogen by peroxide of manganese, the latter does not act by its presence alone (as used to be thought), but by an extremely rapid series of oxidations and deoxidations.

Secondly, in all those cases of catalysis with which we are acquainted, the catalyzer does not increase in quantity during the process; an extremely small proportion may suffice to bring about the change, but that is the same at the end of the experiment as at the beginning. This is a striking contrast to what we know of the marvellously rapid multiplication of the animal viruses, which are, “*ex hypothesi*,” the catalyzers.

But by far the most popular theory of contagion is that which looks upon it as a kind of fermentation, and which has varied its conception of the contagious principle in disease according to the opinions entertained by chemists as to the nature of that process. It used to be included among actions by mere contact or catalysis, so that the partisans of the “*zymotic*” theory held fundamentally the same position as that now occupied by Dr. Richardson; but Liebig’s theory of disturbed equilibrium in the albuminous ferment being propagated to the substances to be fermented soon took the place of the earlier view, while at the same time Cagniard-Latour and Dumas proposed another explanation which is now generally accepted. The French school of chemists taught, what M. Pasteur has since almost demonstrated, that fermentation is owing to the growth and multiplication of some living organism; and this seems to be generally believed, even by those chemists who do not follow M. Pasteur in holding that these organisms act by absorbing oxygen from the fermenting body.

If we suppose the contagious principle of disease to be a living organic ferment, most of the phenomena to be accounted for are explained in a very satisfactory manner. The extreme rapidity of diffusion of infectious diseases; their generic likenesses, which have led us to form them into groups, and yet their power of reproducing only their own kind; the long periods during which they may remain dormant, and the external conditions which cause or increase their activity—all accord with what we otherwise know of the habits of the least perfect forms of organized life. There is, moreover, some direct proof that one contagious disease can be produced by a fungus, in the case of measles, which Dr. Salisbury has seen produced by mouldy wheat straw, and Dr. Kennedy by mouldy linseed meal. We purposely pass over the numerous instances in which low organisms have been discovered in the blood or secretions of the victims of epidemics, because, granting that many of these observations are correct, they are so imperfect, and in some respects so contradictory, as to afford no positive basis for any scientific inference.

We fear that we shall please no one when we say that, as we at present understand the facts of contagion, no single explanation seems to us satisfactory, and that, with certain limitations, each of the three theories we have recited may be applicable to some of the infectious diseases.

Thus, Dr. Morris's germinal-matter hypothesis seems well adapted to account for the occasional virulence of purulent discharges which are not usually contagious; and it *may* be of use hereafter, if required, to explain the communication of tubercle or any similar morbid product from one person to another. The diseases thus induced are local, and are transmitted under conditions of such close proximity that there is no difficulty in supposing the germinal matter may be transferred without change to a site precisely like that it has left, and may therefore continue to multiply as before. And indeed, since Cohnheim's researches have established that the white blood-corpuscles can pass outwards through the capillaries, it is possible that similar cells may enter the blood-current from without, and thence infect the whole organism; but this is mere conjecture.

If, however, we confine our attention exclusively to those general diseases which are ordinarily contagious, we shall find *two groups* among them, separated by very distinct characters. 1. In one of these the cause of contagion seems to be a poison derived immediately from man, or from one of the lower animals, and to which we may fitly apply the term *virus*. The best examples of this group are puerperal fever, plague, variola,

varicella, and vaccinia; syphilis, malignant pustule, glanders, farcy, and rabies. It will be seen that the majority of these viruses are derived from the lower animals, and are mere exotics in our race.¹ The two which are apparently of human origin—puerperal fever and plague—are produced by decomposition of animal matter; we need not dilate on the evidence as to the former, while Griesinger teaches us that plague in Egypt was owing to the careless burying the dead in that country, and has ceased since improvements were made in this respect.

The viruses are able to retain their properties for a considerable length of time when dried, but are always most active when recently produced. For, with one or two apparent exceptions, they seem to require an actual wound in order for them to enter the body; they are the inoculable diseases “*par excellence*.” After inoculation, local action in the wound precedes infection of the system at large, and in the few instances in which we can follow the course of the poison, it passes through the lymphatics.

Considerable light may be thrown hereafter on the nature of the virus diseases by the study of the analogies which exist between several of them, and certain non-contagious diseases. Thus, Griesinger insists with reason on the similarity between the symptoms of plague and those frequently produced by dissection wound; rabies finds its analogy in snake-bite; and the connection between puerperal fever and pyæmia is close, though ill understood.

It is to the group of viruses (we would suggest) that Dr. Richardson's theory of an animal poison acting catalytically is applicable. We have already referred to his discovery of a special alkaloid—“*septin*”—in pyæmia; and in like manner Schlossberger has discovered an alkaloid in the German sausages which produce botulism; the active principle of another animal poison, cantharidine, can likewise be isolated chemically. These analogies would seem to imply that the viruses are not living cells, and that, in the cases where a peculiarly rapid formation of cells is noticed, that is a symptom, and not the essential cause, of the disease. We are reduced to more or less probable conjecture as to the physical nature of the viruses, for the results of direct experiment are somewhat doubtful. On the one hand, M. Monoyer succeeded in vaccinating children with matter which had been repeatedly filtered until nothing could be detected in it with the microscope; but, on the other, the more recent and careful experiments performed by Chau-

¹ Syphilis would be no exception, according to Ricord, and varicella would follow the analogy of vaccinia and variola.

veau go to prove that the poison in vaccinia, variola, and glanders is contained in the solid constituents of the pus. The passage of the poison, in several of these diseases, through the lymphatic glands, would seem to imply that it is a liquid.

2. But there are several contagious diseases which are distinguished from the viruses we have just described by very marked characteristics. In the case of cholera, for instance, it is almost demonstrated that the poison, though contained in the morbid excretion, is harmless at the moment of leaving the body, and developes its properties afterwards when in contact with decomposing organic matter. We have no evidence of transmission of the disease by inoculation, or by mere contact; all we know of it leads us to the belief that it is only acquired by swallowing the poisonous substance. Local action does not appear to precede the infection of the whole system, nor do the lymphatics and their glands seem to be the channel through which the poison travels. The frequent epidemics of the disease are owing, probably, to a fresh formation of the poison under certain climatic conditions of heat and moisture, and contamination of the drinking water by sewage; the last cause being the only one absolutely required to spread the already developed disease. The same causes which breed true malignant cholera, and favour its extension, are capable of producing the non-contagious, milder affections of simple cholera and diarrhœa.

Almost all the above description is applicable to enteric fever, dysentery, and yellow fever, as well as to cholera, the "frustate," non-contagious forms of dysentery and yellow fever being well known; and simple continued fever, febricula, &c., bearing the same relation to typhoid.

None of these four diseases seem to be produced by an animal virus; nay, it is certain with regard to all but enteric fever, and highly probable in that case, that they are often produced afresh entirely outside the living body, under the following circumstances. They are most common in summer and autumn; heat and moisture especially favour their development, and the presence of decomposing animal or vegetable matter is essential.

These conditions are strongly suggestive of the existence of some low forms of fungi; and in the case of cholera, which has been most carefully studied, we seem to be on the verge of proving this to demonstration. With regard to yellow fever, Griesinger, who is usually most careful to avoid any conjecture, believes that it is the strongest instance for the existence of a living miasm as a cause of contagious disease. The harmlessness of cholera dejections when fresh, and their acquiring injurious properties after mixture with impure water, seem to

imply that the cholera poison does not pass through all its life changes either within or without the body; this is the more noteworthy, as the substance may exist in totally different forms in different stages of its existence, and may be, therefore, very difficult to identify. The existence of the non-contagious frustate forms of each of these diseases is probably to be accounted for by a change in the quantity or quality of the fermentible matter, as we know this to be the case with the lower fungi. It may be well to caution the reader against supposing that the active principle in fermentation need always be a fungus; M. Pasteur has shown that the production of butyric acid by fermentation is accompanied (and, he believes, caused) by an infusorial animalcule.

This is a brief summary of the grounds for our belief that the animal virus and fermentation theories of contagion are both true, and err only by seeking to explain every case. In one series of diseases, we believe the infecting principle to be a *virus* of animal origin; in another series we think it is a living *miasma* (if we may thus employ a much abused word) produced originally outside the body, but becoming afterwards capable of development within it.

We, of course, choose only the most striking examples of each kind to illustrate our opinion, but we think that all contagious diseases can be classed under one or other of these heads; and, when this is done, the majority will be found to be viruses. Thus, the poison of pertussis is probably inhaled, and acts on the same nerves as that of rabies; it apparently needs to develop locally in the lung during the first stage of the disease, before infecting the whole system. Scarlatina and measles are most likely viruses, as is evidenced by the inoculability of the latter at least, and by their general resemblance to variola and varicella. The incubation stage is looked upon by Niemeyer as the strongest proof of their miasmatic nature; but it is also to be explained as being required for the process of local action of the virus. The production of measles by mouldy vegetable matter is a serious objection to its being due to a virus; the examples have been so few, that we may fairly doubt if the disease thus caused was true ordinary measles.

Typhus, again, is so independent of climatic conditions, and so evidently connected with dirt and overcrowding, that there can be little doubt it is due, not to a *miasma*, but to a virus, producible "*de novo*," like rabies or plague. The symptoms produced by botulism (German-sausage poisoning) are analogous to those of typhus, and appear after an incubation period of from eight to fourteen days.

We have dwelt at such disproportionate length on what may

appear to be a speculative question, because it seems to us that the present confusion of all the phenomena of contagion under one common cause is the chief source of the misunderstandings and controversies which prevail. We will merely add, that possibly science may hereafter prove that animal viruses are also living cells; in that case the distinction of *origin* between these and the miasmata would still exist.

Dr. Anstie's little book is excellently calculated to spread a correct knowledge of contagious diseases among the public. Our only possible quarrel with it would be that it is somewhat too long for an impatient public, who will seldom devote as much time to acquiring information of importance to themselves, as is wasted without remorse in gratifying idle curiosity or reading a sensation novel.

Griesinger's treatise on infectious diseases has been for a dozen years the principal German classic on the subject, so that its translation into a better known language will be a boon to many readers. Where all is excellent it is hard to say what deserves the greatest attention; probably the description of "bilious typhoid"—a new morbid species established by the author—will interest most readers. We also have here the best account extant of plague, and a very minute and practical history of typhus and enteric fever. The translator's notes are well chosen, and add to the value of the work.

We are sorry we cannot speak so favorably of Professor Virchow's lecture. Perhaps the language is partly to blame, for it has evidently been translated into English by the author himself, and not revised by any competent person; perhaps, also, the illustrious lecturer thought it well to avoid any scientific examination of his subject before a mixed audience. He appears to look upon typhus as the famine-fever "*par excellence*," and considers Murchison's derivation of typhus from overcrowding, relapsing fever from scarcity, and enteric fever from sewage poisoning, as only partially true.

M. Anglada's book is devoted to proving that fresh kinds of diseases are frequently arising, which often attain the importance of pestilences. It is, therefore, almost exclusively a history of the great epidemics which have at different periods devastated the world. M. Anglada is, perhaps, too much inclined to rely upon the accuracy of the accounts of the earlier epidemics, and to form a positive opinion with too slight data; but the volume contains a great amount of interesting matter. With regard to the principal controverted questions, he is of opinion that the pestilence described by Thucydides is the same as that which Galen observed—that measles, scarlatina, and smallpox all made their appearance about the same time; that the

sweating sickness is not a form of plague, nor has any connection with miliary fever; and that syphilis arose for the first time in the fifteenth century. He suggests that, probably, the various causes tending to produce a fresh contagious disease may incubate during a long time before producing their effect. But what those causes may be his book affords us no help in ascertaining, further than by refuting the various *à priori* suppositions which have been put forward.

Perhaps we may be led to discover the causes producing contagious diseases by examining the conditions under which they spread. This has been attempted by Dr. Ransome, who compares the statistics of disease as registered in Marylebone and London in the last few years, and the principal results thus obtained are extremely interesting. Thus, it is shown that there is a remarkable correspondence between the prevalence of most of the ordinary epidemics in these two distant places, whence the author rightly concludes that "neither imperfect sanitary conditions nor simple contagion will account for all the phenomena of the production and propagation of epidemic disease." This influence may either be such climatic conditions as are favorable to the development and propagation of miasmata; or it may render persons more apt to receive the influence of the poison. But Dr. Ransome is able to go still further, and to show that epidemics, affecting chiefly certain organs of the body, are most common at the times when the non-contagious diseases of those organs prevail: thus pertussis and measles are most common in winter and spring; scarlet fever is most frequently seen in autumn, when ordinary sore throats are most common, and epidemics of enteric fever coincide or follow a great increase of diarrhœa. The explanation suggested is, no doubt, the correct one, that weakness of particular organs affords an entry to the poison which is taken into the system at those points: the author might have quoted the case of cholera induced by an accidental diarrhœa. We hope to find Dr. Ransome continuing his work; when he has more abundant materials at his disposal, he is evidently on the right track for obtaining most important results.

Dr. Angus Smith's little book on 'Disinfectants,' and the review of it in the *Sanitary Record*, are striking examples of the prevalent confusion as to the nature of contagion in disease, since the former proceeds on the assumption that this is always a living germ, whilst the latter thinks it is an alkaloid poison. We are quite inclined to agree with the reviewer that Dr. Angus Smith's position as a patentee of one of the disinfectants should have disqualified him from being employed to examine their respective merits; and we think with him that

Condy's fluid, chloride of lime, pure chlorine, are unduly depreciated in his report.

Our own belief is, that carbolic acid should only be employed, as Dr. Barker suggested, for disinfecting fluid or solid excreta. Even in that case it should be borne in mind that Pettenkofer has shown that this antiseptic will only suspend the activity of ferment cells for the time of its presence; and unless the offending matter be at once removed, we should prefer Condy's fluid or chloride of lime. Dr. Barker thought highly of iodine; and Dr. Richardson also employs it as a destructive disinfectant. If, however, the animal viruses are true alkaloids, it is perhaps inferior to chlorine or bromine; since these two break up the alkaloids, while iodine combines with them without decomposition. The metallic salts, such as chloride of zinc and sulphate of iron, the favorite disinfectant in France and Germany, are to be avoided, because, unless highly concentrated, they probably only act by destroying sulphide of ammonium, which we know not to be the contagious principle, and which serves as a constant reminder of the presence of unwholesome effluvia.

XII.—Indian Sanitary Inquiry.—The Hurdwar Pilgrims and Cholera.¹

SINCE the establishment of the three Sanitary Commissions of India, the commissioners of Bengal, Bombay, and Madras have separately published annual reports of the health of the European and native troops, of the prisoners in jails, and of the native populations. These reports are valuable contributions to our knowledge of disease in India. The statistics of the health of the European troops and prisoners have been pretty well made known by the departmental reports of the army medical service, and of the inspectors of jails of the different provinces, but the knowledge of diseases of the native populations has been much less diffused. The reports of the Sanitary Commissioners enter as fully as can be done at present into the question of the health of the millions of our Indian fellow-subjects. As yet the information is imperfect, but statistical inquiries and systematic records have now been commenced, and in a few years we may hope to be properly informed of the movement, forms, and proportion of disease among the whole population, both native and

¹ *Fourth Annual Report of the Sanitary Commissioner with the Government of India, 1867.* With Appendices containing Returns of Sickness and Mortality among the British and Native Troops, and also among the Prisoners in the Bengal Presidency for that year. Calcutta, 1868.

European. Until such knowledge be obtained our sanitary measures will want completeness and efficiency.

The volume before us was written by Dr. Cunningham, officiating sanitary commissioner with the Government of India. It reports on the health of the Bengal Presidency for 1867, and illustrates the truth of the above opinion of the value of accurate knowledge of the movement of disease among the masses of the native populations. The year was remarkable for a severe epidemic of cholera in the north-west provinces of India and the Punjâb. A large part of the report, 332 out of 462 paragraphs, is devoted to this subject. Dr. Cunningham has narrated with considerable detail and impartiality the known circumstances of the epidemic, and considering the difficulty of following up the history of a disease in a country like India, he has been very successful in his efforts. As the section connected with the Hurdwar pilgrims forms such an interesting feature of the report, and as its subject is of such great practical importance, we propose to direct our attention principally to it.

Popular opinion supposed that the epidemic which passed over the upper part of the north-west provinces and the Punjâb originated at Hurdwar, during the celebration of a Hindoo religious festival, and spread from thence over the affected territories. Before proceeding to test the probability of this opinion, it will be well to take a brief survey of the occurrences at Hurdwar. Hurdwar is a town in Northern India, at the base of the Himalayas, on the spot where the Ganges begins its course in the plains. It is the seat of a great Hindoo pilgrimage, which takes place annually in April. It is supposed upon good data that, in 1867, one of the specially sacred years, from two to three millions of people were assembled there during the fortnight that the fair lasted. These were encamped on a low flat island in the Ganges, about nine miles long and three miles broad, and on the neighbouring banks of the mainland. The actual area of encampment was supposed to be about twenty-two square miles. Bathing in the sacred stream on a certain most propitious day, is the object of the devotees, but a portion of them, not a large one, bathe and depart before the principal day. In 1867, this special day was the 12th of April, and from the noon of that day the pilgrims began to disperse rapidly—on the morning of the 15th the encampment was quite deserted.

The health of the camp during the assemblage was very good, but few persons were treated as in or out patients of the hospitals, and among these only 101 for diarrhoea and 79 for dysentery, during the whole period of the fair. There were a few cases of smallpox; they were separated from the rest of the community, and

the disease did not spread. But in an immense camp, such as this, it is very probable that some disease, especially among the rich and their followers, escaped the notice of the authorities. One case of cholera, not fatal, was treated in hospital on the 9th. On the 13th of April eight cases, and upon the 15th nineteen cases in all were admitted into the hospitals.

The weather was the usual hot weather of the north-west provinces in April; very hot in the day, but modified at night at Hurdwar, by cold air coming down from the snow-capped mountains, through the gorge in which the river runs. On the afternoon of the 11th heavy rains fell, and continued all night, with great reduction of temperature, drenching the ill-sheltered pilgrims. Some rain also fell on the morning of the 12th.

The supply of provisions was sufficient and good. The drinking water was obtained from the river, and from a few wells sunk in the soil.

The Government for the first time undertook to enforce sanitary arrangements at the fair. The difficulty of arranging for this vast multitude may be conceived when we consider how troublesome conservancy is for seven or eight thousand people at Wimbledon, even with the help of the appliances of England. At Hurdwar the population was at least two thirds of that of London, and probably more, and the appliances those of rural India only. The civil officers declared that they could not cart away daily the 446 tons of matter, the minimum quantity which it was calculated would have to be removed. Dr. Cutcliffe, the sanitary officer of the fair, was therefore driven to dispose of the sewage on the spot. This was deposited in trenches four feet deep by one broad, conveniently dotted over the encampment. Two trenches being dug at a time, the earth of the second was used to cover up the excreta in the first. These latrines were not roofed in. The soil was generally sand and clay, but in a small part of the camp it was of sand and boulders, and there the trench system could not be worked. In this part the refuse was burnt in furnaces. The camp was reported to be remarkably free from smells, and to the senses the arrangements appeared to be successful, the cleanliness affording a great contrast to the conditions of former years. As before said, all went on smoothly during the encampment, a few cases of cholera only having shown themselves between the 13th and the morning of the 15th during the dispersion of the pilgrims, who suffered, however, almost immediately after they had left the camp. Such is a brief outline of the state of matters at Hurdwar.

It was at first currently believed that the outbreak happened during the time of the fair, as in 1783, but Dr. Cunningham

shows that this was not the case. He believes, however, that the pilgrims were infected at Hurdwar, and endeavours to account for the origin of the infection. He does not adopt the theories of spontaneous generation in filth, general atmospheric, meteorological changes, and the like. He favours the opinion that the specific disease was introduced into the camp from without, probably by pilgrims from the neighbouring districts going late, as is their wont, to the fair. In support of the probability of this there is the testimony of the superintendent of the neighbouring Terai (districts lying at the foot of the hills), that pilgrims going to Hurdwar from his district had cholera, and that several of them died on the road. There were nineteen deaths at Bazpore, fifty miles from Hurdwar, between the 5th and the 12th of April inclusive. Dr. Cunningham thinks that cholera excreta may have been buried in the trenches, and that carried by the drainage of the heavy rain cholera matter soaked or passed into the river, and was swallowed by the pilgrims on the great bathing day of the 12th; for to drink of the water of the Ganges, as well as to bathe in it, constitutes part of the ceremonies to be gone through. Dr. Cutcliffe, whose opinion coincides with that of Dr. Cunningham, has furnished for the report an interesting account of the bathing and drinking processes, showing that if the water was contaminated there was no difficulty in accounting for its entrance into the system. Dr. Cunningham inclines to the view that the poison was taken into the system in this way, and that after various periods of incubation it showed its effects upon the pilgrims on their way to their homes.

Granting, for the sake of argument, that infection of the mass of the people took place at Hurdwar, we think that sufficient proof has not been given of its causation by contaminated water. There must be great difficulty in proving such to be the case in such a locality. It is not the question of a pipe or a conduit or a well, through cracks or fissures in which contamination of water supply can be traced with certainty. It can only be supposition, therefore, and not demonstration, that faecal matter passed into the river from the trenches. We think that in the absence of observations made at the time on the water level, on the distance of the trenches from the river, on the chances of water overflowing them or running directly from them into the river, or on the relative position of the somewhat limited bathing area to the ascertained sources of pollution, and of the strength of the stream—that is, without direct observation of the fact of faecal contamination of the water, and of that water coming within reach of the bathers of the 12th, it would be impossible to lay much stress on the probability of the poisoning by water. It

would be impossible to make any satisfactory observations upon the subject after the event. We do not see why aërial infection should not be entitled to equal consideration with aqueous poisoning. If the poison could stream into the river, surely the specific poisonous matter, whatever it may be, might rise into the air and reach the mass of people as readily as the water did.

Though it may not be possible to prove the mode of infection at Hurdwar, the elements of poison seem to have been present. There was the source of specific cholera-material in the neighbourhood, there were the means of its introduction into the camp by pilgrims from the infected places coming to the fair, there was the excrement-charged soil which was ready to pollute air or water, and there was the unfortunate fall of rain to assist in the decomposition of the excrement. If ever cholera dejections had a fair chance of being noxious, we should suppose that it would be under such conditions as these. The manner in which these elements operated cannot, we fear, be followed. Although we do not see our way to accept the water-poisoning as proved, we cordially agree with Dr. Cunningham that on any future occasion the excreta of the pilgrims should not be allowed to accumulate beneath their feet, and that fæcal refuse should not be burnt in neighbouring furnaces. It is obvious, whatever view may be entertained respecting the origin or spread of cholera, that all excremental matter should *coûte qui coûte* be removed far from the precincts of the pilgrim camps.

All this while we have assumed that infection originated at Hurdwar, but it has been questioned whether it really did so originate, and was carried onwards by the pilgrims. By some the pestilence has been attributed to the usual development of cholera in an epidemic season, and it has been supposed that the pilgrims were the recipients and victims of the epidemic influence, whatever that may be, rather than the bearers and spreaders of the poison. It is true, that some parts of the north-west provinces were not exempt from cholera during the latter part of 1866, and the beginning of 1867. It was at Agra in the end of 1866, and in the Delhi district in December. It was creeping along the country at the foot of the hills in February and March. There was cholera at Agra early in April, and a few cases occurred here and there about the time of the epidemic. We have seen that it was at Bazpore, not fifty miles from Hurdwar, early in April. Reasoning from such facts as these, and from close study of previous epidemics, Dr. Bryden, Statistical Officer of the Bengal Medical Department, had predicted that an epidemic of cholera would occur in the spring, and warned the sanitary commissioners to be prepared for it. There was, it has been said, an epidemic influence brewing, and why should not

this have caused the epidemic,—as might be expected,—telling fearfully upon the exhausted and debilitated pilgrims, who would be predisposed and easy victims. Granting that there were some unusual conditions,—seasonal conditions perhaps,—favouring the spread of cholera, we have to inquire whether the facts agree with the theories of origin at Hurdwar, and communication by human intercourse, or otherwise.

It is impossible within our allotted limits to follow Dr. Cunningham closely in his copious details of the proofs of the spread of the disease through the means of the pilgrims, and of its progress from Hurdwar itself. We must content ourselves with examining the results of his labours.

The Sanitary Commission set itself to work earnestly and carefully to ascertain from the medical and civil district officers, and the military and prison authorities, the history of the progress of the epidemic. The result of the inquiries is, that, as we have already said, the disease began to show itself, in the comparatively deserted camp, between the 13th and the 15th of April, though the cases were but few. The succeeding cases were in the localities and districts immediately surrounding Hurdwar, and from this centre the epidemic area widened in nearly all directions. Further, the pilgrims were almost always the first persons attacked in any locality, and cholera attended them in their route wherever they went. Of course, after a few days, the identification of the propagation of the disease through pilgrims was lost, but for the first week, and over an area of from fifty to 300 miles distant from Hurdwar, the facts seem clear enough. It is quite possible that pilgrims may have been infected at Hurdwar, and yet have got to a considerable distance before they succumbed to the disease. The period of incubation may last many days, and the death struggle does not always set in immediately that the symptoms begin. There may be many days of diarrhoea before the collapse comes on, and thus, what with the incubation and what with the diarrhoea, a man may be carried over eight or ten days of a journey before severe symptoms prostrate him. There are facts on record which make it probable that the incubation period may last longer than this; as in the case of the transport "*Renown*," sailing from Gibraltar in 1865. The appearance of confirmed cholera in a pilgrim a week or ten days after he had left the fair, does not militate against the opinion of the disease having been received there.

Dr. Cunningham's report shows that of the fifty-one districts, comprising the area of this epidemic, no cholera appeared in any locality, except in one, Goorgaon, where too the history of the first case is doubtful, until ample time had been given for the

pilgrims to reappear in it, or for others to enter it from infected places, and, indeed, until the pilgrims actually had returned. Excluding all cases about which there may have been any doubt, in thirty-five out of the fifty-one districts, there is the clear statement of the medical or district officer, and sometimes of both, that the first persons attacked were pilgrims, and after they had been seized the disease appeared and spread among the residents. The dates of the outbreak of the disease, coinciding with the appearance of the pilgrims, nearly all the medical officers, thirty-two in number, residing in the track of the disease, believed it was brought into their districts by the pilgrims. Dr. Cunningham states that the exceptions were few, and that in no instance was positive evidence advanced to show that such a cause was improbable, much less impossible. Although we think that in a few instances the inferences drawn in some of the reports of the medical officers might be questioned, yet the positive evidence is generally very strong and satisfactory in favour of the introduction of the disease into places by the pilgrims.

It has been objected against the origin of the cholera at Hurdwar, that, though the pilgrims were the carriers of the disease, they merely caught it up in infected districts as they went, and distributed it in their progress, as an army, or other moving mass of men might do. We think that this interpretation is hardly compatible with the apparent development of the epidemic from the centre from which these travellers started. In the distant circles of invasion the pilgrims may have been infected by their comrades or others, and probably they were so, and carried on the virus as from fresh centres. After careful attention to the facts of the report, and assuming their correctness, we think the probabilities are in favour of infection of the mass of the people at Hurdwar, and of their diffusion of the disease as they went along. The pregnant facts collected from this interesting history are, that cholera attached itself, as it were, to the pilgrims, and showed itself first among them, that it followed their lines of route only, that it did not outrun them, that their progress was its progress, and their limits its limits.

It seems fair to conclude, therefore, that whatever may have been the epidemic character of the year, or the unknown causes which may have favoured the development of the disease, human intercourse spread it in this particular instance. Dr. Cunningham says that it is possible that the facts may be to some extent incorrect, but that it is quite impossible that the whole story of the returning pilgrims carrying cholera with them from Hurdwar to Rawul Pindee, with the dates of its appearance in the successive districts into which it penetrated, could be invented. We

think so too, and that, without straining conclusions, we may fairly consider this epidemic to have been propagated by pilgrim movements. It may be too much to assert that cholera would not have appeared in the north-west and the Punjâb, without the occurrences of the Hurdwar fair, but it would probably have been later in the year, and, perhaps, less severe. The facts recorded seem to furnish additional proof of the propagation of cholera by human agency, and, we believe, no one would question this if, instead of being one of cholera, the epidemic had been one of scarlatina or of smallpox. We do not mean to say that contagion operated in the sense understood of old, but that man was in some shape or other the bearer and distributor of the poison. This is, we believe, the most carefully and fully worked out history of a cholera epidemic among the native population of the Bengal Presidency yet published, and we hope that the Sanitary Commission of Bengal will, on account of the great practical consequences involved, pursue the inquiry with equal care on future occasions. The present report furnishes a valuable chapter to the history of cholera.

The epidemic was a very formidable one. It extended over the upper part of the North-West Provinces and the Punjâb, and passed beyond our frontier. Dr. Cunningham considers that the propagation by means of pilgrims could not be traced further than Shajehanpore on the south-east, and Aligurh on the south, but that it could be followed much further on the north-west line. It is remarkable that the communication by pilgrims could not be traced on the south line towards Agra beyond Aligurh, though without doubt pilgrims travelled in that direction. This is an inexplicable circumstance in the present state of our knowledge, but it is necessary to mention it as well as the facts of opposite character which have been so fully dwelt upon. The mortality by cholera among the whole native civil population of the north-west provinces and the Punjâb has been estimated at about 117,181. Although we have mentioned the effects upon the native civil population in such detail, it must be understood that the epidemic attacked all classes of the community.

Dr. Cunningham notices the comparative immunity from cholera of the jails in Upper India, although many were in the centre of an infected population. Out of seventy-six jails in the Upper Provinces, Oude, and the Punjâb, fifty-one escaped entirely. In these jails a modified quarantine has been imposed on all new prisoners, and this was more strictly carried out in times of cholera; that is, made as stringent as imperfect means allowed. If, however, the comparative immunity of jails from cholera is allowed to tell in favour of enforced isolation, it must

not be forgotten that the native troops, without any such protection, suffered much less in the mass than the European troops, and, with rare exceptions, much less than the European troops in the same station; the only recognisable difference between them bearing on this exception being that the native troops live in huts instead of barracks; but even this difference is not so constant as it used to be, and further information on their dwelling accommodation is to be sought for.

Cholera was prevalent in other parts of Upper India besides those included in the area of the epidemic attributed to the pilgrims. There seem to have been other centres. It began at Bhurtpore on the 6th of April, at Agra on May the 30th, at Allahabad in March and April, became epidemic from the 20th to 30th April, and proceeded along the right bank of the Jumna to reappear at Allahabad on the 28th of July. It broke out in the central jail of Lucknow on the 27th of March. In the city of Lucknow there were three cases in March and April, and the maximum was reached in August. The central provinces entirely escaped.

The Government took every possible precaution to arrest the spread of the disease by sanitary and restrictive measures. The pilgrims were prevented, as far as possible, from mixing with the inhabitants of the districts in which they passed. Medicines, medical attendance, and even food, were provided for the pilgrims. Conservancy of these camps was carefully attended to. It is impossible to say what success attended these measures. Opinions were divided. Some officers thought that matters would have been worse but for the measures, others did not attribute much good to them. It is satisfactory to think that whatever could be done was done. Land quarantines, and even prevention of intercourse in any shape must be very difficult to carry out with a couple of millions of people to contend with, and where the streams are sometimes so great that they represent moving cities rather than anything else. Sixty thousand pilgrims passed the Guggur Mill in one day. Quarantine in such circumstances as these is hopeless. With hygiene we are not much better off. It will be nearly as difficult to educate the masses in hygiene, or to enforce it, as to bar out thousands of pilgrims by quarantine. For the present, while these immense pilgrimages go on, nothing is so likely to be useful as stringent and carefully enforced sanitary regulations at the shrines. When the number of pilgrims is smaller they may be more effectually dealt with, both by restrictive measures and hygiene.

After the severe epidemic of cholera of 1861, Government, on the advice of the Commission which investigated the outbreak, issued orders compelling all regiments to move into camp

when the disease threatened to become epidemic. This plan was reported to have been useful in the instances in which it was adopted in 1861. It was again largely tested in 1867. Dr. Cunningham concludes that the moving into camp was on the whole favorable. It must be admitted, however, that there were instances in which it was not efficacious.

We have no space to notice the many other valuable portions of this report, and must close with a brief reference to the health of the European troops. The admissions into hospital were less in 1867 than in previous years. It was 53 per 1,000, 58 per 1,000 as the average of 1866, and 90 per 1,000 as that of 1859. The death-rate 30·95 in 1867 to 20·11 per 1,000 in 1866. The mortality from cholera, which in 1866 was 1·37, amounted to 13·84 per 1,000 in 1867, and explains the great difference in the death-rate of the two years. The invaliding was 47·28 per 1,000, two-thirds being for change of climate, and one-third for discharge. We deeply regret to observe that the deaths to admissions in cholera still maintains the high rate which it has attained of late years, and which far exceeds that of former periods. The mortality so treated was, in 1867, 66·07 per cent., the average for the past seven years being 66·94 per cent.

The appendix contains Dr. Bryden's statistical tables in 1867, arranged under the following heads: 1. European troops; 2. Native army; 3. Jail population; 4. Detail of admissions and deaths of the European and native armies, and of the jail population. These tables contain the fullest information on all matters of health reducible to figures, and deserve the most attentive study.

Bibliographical Record.

Flora.—Climate of Egypt.¹

THE contents of this small work are of a somewhat varied character. The first chapter is occupied with a notice of Suez as a packet-station, of the public companies connected with it, of its population, its institutions, &c. Its medical residents consisted of two physicians—one a European, the other an Arab, employed by the Isthmus Canal Company; of a physician and surgeon engaged by the English authorities; of a physician belonging to the French company, and of a sanitary physician, an Arab assistant surgeon, and a trained female attendant for women, employed by the Egyptian Government. Dr. Flora occupied the post of sanitary physician in Egyptian pay. During the time he held this position the Abyssinian war was brought to a close, and large numbers of troops and camp-followers passed through Suez and brought a large augmentation of work upon him. He was consequently well-placed for observing the effects of the climate of the locality upon others than the indigenous population, and also the prevalence and diffusion of disease both among the stationary and the shifting inhabitants of the town and neighbourhood.

The chapters following are reports of observations with comments, based on personal experience.

His second chapter deals with the ulceration of the feet and legs observed during the first half of 1868 among those coming from Massowah and Zula, commonly known as the Yemen sore or ulcer. This lesion is met with on the seaboard of the Red Sea, and also in Mozambique and Cochin China. The author narrates several cases in illustration of its characters and progress, and gives woodcuts in further elucidation of its ravages. From the histories given, the disease appears to be a "rodent ulcer," rebellious under the treatment adopted to arrest and cure it. Neither a syphilitic nor a scorbutic origin belongs to it according to Dr. Flora's experience, and it is in his opinion a consequence of great variations of tempera-

¹ *Aerztliche Mittheilungen aus Aegypten.* Von Dr. ANTON FLORA.

Medical Memoranda from Egypt. By Dr. A. FLORA. Vienna, 1869. 8vo. Pp. 105.

ture by night and day, and of exposure. It happens almost exclusively among the labouring classes, to whom such exposure is common.

The two succeeding chapters are chiefly made up of entries from the author's official documents as a sanitary medical officer and physician of the Arabian Hospital in Suez. The first of the two is on the choleraic maladies that so extensively prevailed in Egypt in 1865-66. Dr. Flora states that the cholera was introduced by the Mecca pilgrims, and first made its appearance in Alexandria. The second of the chapters mentioned is occupied chiefly with statistical tables and abstracts from them, and shows the number of admissions during a year into hospital, the nature of the disease on admission, and the termination of several cases.

The table of the relative frequency of the several maladies received into the hospital exhibits the curious fact that, in the dry climate of Egypt, rheumatism is one of the most prevalent complaints, and is, in fact, only surpassed by fever. It is described as frequently associated with diarrhœa, and is not deep-seated or severe; very rarely complicated with heart disease although palpitation is common. A favourite remedy for rheumatism with the natives is camphorated oil. Gout is almost unknown, except among the European population.

Observations on the action of the Egyptian climate on Europeans is the subject of the fifth chapter. Healthy individuals are liable to constipation during their sojourn in the winter, but as spring advances the hot days and cold nights are productive of diarrhœa and rheumatic affections. During March, April, and May sharp attacks of diarrhœa, dysentery, intermittent fever, dyspepsia, and slight continued fever are common; and in the last named month eye affections become rife. With June come in skin eruptions; lichen tropicus, boils, and small cutaneous abscesses, which are considered salutary. The proclivity to cutaneous maladies is influenced by the activity of the skin; the more freely the skin perspires the less do individuals suffer. The same holds true, also, with regard to the tendency to diarrhœa and dysentery. During summer diarrhœa continues to prevail, but when autumn (September) approaches gastric derangements and intermittent fever again become rife, and so continue until the middle or end of October, when the healthy season commences.

Invalids resort to Egypt for the relief of gout, asthma, chest diseases, obstinate wounds, and carious bones. Gout is benefited, and though paroxysms recur they become milder and less frequent. Asthmatics, having emphysema, suffer from the cold of the winter when the winds are strong and there is considerable mixture in the air, but if free from tubercle are, in course of time relieved. Carious bones and obstinate ulcers very soon exhibit the beneficial influence of the climate, and syphilitic disease is very amenable to treatment.

Cardiac affections, where there is no dropsy, derive benefit, but when dropsy has supervened become speedily worse.

The characteristic of the Egyptian climate is its power in promoting the rapid retrogression of hyperæmia and the absorption of exudations in most organs of the body, excepting the skin, intestinal canal, and liver, which are exposed to increased action and hyperæmia. As a curative agency it is active in lung and kidney diseases; in chronic pneumonia, long-standing hepatization, emphysema, stationary tuberculosis, over-activity of blood-genesis, and in gout. It is contra-indicated where the skin is harsh, dry, and unperspirable, and where, in disease, the nervous force is sinking.

Dr. Flora divides tubercular cases into three classes in respect to the effects of the Egyptian climate upon them. His first class comprises patients who, in the course of chronic diseases, assume the features of consumption, and in whom auscultation and the urine reveal the continued persistence of inflammatory action, but the recognised tubercular habit is wanting. The second class includes patients with stationary tuberculosis, in whom either the auscultatory signs of consumption are not marked, although the disease is sufficiently revealed by the general habit and by respiratory disturbance, or extensive dulness exists, but without fever and without inflammatory attacks. The third class is made up of cases of tuberculosis in which there is fever, occurring periodically, or otherwise is continuous.

Patients of the first class are almost always cured; not only is the morbid deposit removed, but also a healthy blood-making process re-established. The like benefit cannot be so distinctly predicated respecting patients of the second class. Improvement is usually felt, but Dr. Flora noticed often that they fell victims to the febrile attacks of spring and autumn, often in from three to four weeks. Lastly, most of those belonging to the third category find their grave in Egypt, an event that happens frequently in the period between March and May. Such cases should not be sent to Egypt, or, if there, should quit the country early in March.

The last chapter on the climate of Egypt is brief and contains no special information of importance derived from the author's own observations. He divides the country into three zones:—1. The coast line of the Mediterranean, extending twenty miles inland,—or the northern half of the Delta; 2,—the southern half of the Delta, with Cairo and the country included within two geographical degrees south; and 3,—Upper Egypt as far as the Cataracts. The first zone is cooler and without rain in summer, but all the coast towns are damp and unhealthy in winter. Dr. Flora's conviction is that, with reference to healthfulness, it is not those places that exhibit the slightest variations of temperature that possess that quality most largely.

The chapter is brought to a close by some tables of the temperature prevailing at Port Said, Ismailia, and Suez in 1866 and 1867.

Coulson on Syphilis.

OUR knowledge of syphilis has made great progress of late years. There are few subjects to which we could point that have advanced more rapidly ; and the number of treatises upon the venereal diseases which have lately appeared seem to confirm this statement. During the last few years a constant succession of works has issued from the press both in this country and abroad. Some of these works have thrown fresh light upon the subject, and have advanced our knowledge in no considerable degree, while others have gathered up what was already known, and presented it to us in a convenient and concise form. To this latter class the volume before us belongs. It does not profess to add anything to our previous knowledge. It does not even aim at being a full and complete account of the venereal diseases. It is not so original or scientific as the writings of Ricord, or Lancereaux, or Diday, or Lee, or Hutchinson. It is not so full and complete as the works of Bumstead or Berkeley Hill. But, as far as it goes, it gives a good account of the main features in the pathology and treatment of syphilis.

The volume consists of lectures which were delivered at the Lock Hospital, and, as we might suppose from the form in which the work is cast, it does not enter very fully into the subject. It is hardly possible, even if it were desirable, to discuss all the details of a complex question, such as syphilis, in a short course of lectures. The intricacies of a complicated and very difficult subject may be studied to more advantage in the pages of a systematic treatise ; and Mr. Coulson has exercised a wise discretion in not attempting to enter into all the details of his subject in the limited time at his disposal. It would have been impossible to have done so. He has therefore adopted a better course. He has given us a clear account of the progress of the disease ; he has explained the pathological views which are held by a large number of modern syphilographers ; and he has supplied excellent rules to guide the practitioner in his treatment.

Though his definition of terms is not very happy, we have no difficulty in learning that our author is what is generally called a *dualist*—that is to say, he holds that there are two distinct kinds of vene-

¹ *A Treatise on Syphilis.* By WALTER J. COULSON, F.R.C.S., Surgeon to the Lock Hospital, and to St. Peter's Hospital for Stone and Urinary Diseases. London, 1869. Pp. 373.

real sore—the soft and the hard chancre—each of which is caused by its own specific poison, and this doctrine he lays down somewhat dogmatically. He has evidently no doubt about it himself, and he wishes to leave no doubt about it in the minds of his auditors. Indeed, he enunciates the “laws” which he considers govern the evolution of syphilis with such force that no one would suppose for a moment that there are some very distinguished surgeons who altogether discredit these *laws*. And herein lies the weak point of his work—it is not a philosophical statement and estimate of the whole subject. It is a book founded upon a particular view, held by a particular school. It may serve to set forth the pathological opinions of this school of surgeons, and it may supply an excellent guide to treatment, but it will not take its place as a scientific essay on the whole subject.

There are many interesting cases recorded in these pages, some of which are calculated to throw light on obscure points connected with the subject. Here is one which shows how rapid is the systemic infection which follows the absorption of the syphilitic poison, and how little hope we have of preventing secondary symptoms by the earliest and most complete removal of the chancre itself.

“Some years ago a patient was admitted into the hospital in the Harrow Road, with a small but well-marked indurated chancre, at the extremity of a somewhat elongated prepuce. I performed circumcision with a scalpel that had just returned from the instrument-maker, and brought the edges carefully together with sutures. The wound presented a healthy appearance for the first three days, and the sutures were removed; but subsequently the cut surfaces became indurated, and constitutional syphilis followed in due course, apparently in no way modified by the operation.” (p. 42).

The therapeutic value of the iodide of potassium is discussed at some length, and the extent to which it may be pushed. Here is a case which shows how efficient but how transitory is its power over the disease, and how long it must be continued in order to produce any permanent effect.

“Some five years ago a gentleman came under my care for syphilitic sarcocoele. He was treated with the iodide, and soon recovered. A few months afterwards he got married, but continued to take the remedy, according to my advice, once a day, for some time. I had lost sight of the patient for two years, when he called upon me, and gave the following history. The testicle had remained perfectly well, without any tendency to enlarge, nor had any other symptom of syphilis appeared, but he found it quite impossible to leave off the iodide. On the first attempt he discovered that his general health began to decline two or three weeks afterwards; he became weaker, his appetite failed, he lost weight, and night-sweats ensued. As he happened to reside in the country at this time, he resumed the iodide without consulting

any medical man; the appetite at once improved, and he quickly recovered his usual health. He now repeated the same experiment, but under the advice of a physician, who ordered him to go to the seaside and to take quinine with iron. The change of air and of treatment was not attended by the results expected; the symptoms already described began to manifest themselves again, and the patient was again compelled to resume the iodide. He had also tried various baths and waters on the Continent, amongst others those of Aix-la-Chapelle, but always without effect. I advised him to make an experiment, and to try what was the smallest dose necessary to keep him in health, ordering three grains of the iodide, fifteen drops of the aromatic spirit of ammonia, and one drachm of tincture of orange-peel, to be taken in water every morning. This small dose was sufficient to keep the patient in excellent health, while it might be omitted for a short time without inconvenience; but any lengthened omission was sure to be followed by the usual results. The last experiment which the patient made upon himself was in the summer of 1868, at St. Leonard's; but on this occasion he experienced what he considered as an attack of rheumatism, in addition to the general debility and night-sweats, &c. The iodide of potassium, in five-grain doses, removed the pains and other symptoms." (p. 245).

Mr. Coulson's work is confined, as its title imports, to syphilis alone. It does not include the whole of the venereal diseases. And here we cannot but think that the author has erred in judgment, and that it would have been better to have included gonorrhœa. There are many interesting questions arising out of the points of contact between the different forms of the venereal disease, and these the author has necessarily excluded from his consideration. Nor has he taken up all the subjects which properly fall within the chapter of syphilis, for he has passed over altogether the important questions of hereditary and congenital syphilis. As far, therefore, as the discussion of syphilis alone is concerned, the volume before us is far from being complete. It is marked, too, by a good deal of repetition which might be allowable, and even desirable in the lectures at the time of their delivery, but which ought to have been omitted when they were prepared for publication. If these repetitions had been eliminated, and if the space thus gained had been devoted to a brief account of hereditary syphilis, as well as of gonorrhœa, the work would have been made more complete and more valuable.

Wood's Notes on Metals.¹

THE most noteworthy facts concerning the chief metals are collected and arranged in this small volume. First of all, a few pages

¹ *Notes on the Metals.* By THOMAS WOOD, Ph.D. London, 1868. Pp. vi, and pp. 180.

are devoted to the physical properties of the metals; then we have a brief and general account of the compounds of metals; then metallurgy is alluded to. The theory of salts and radicals comes in for its share of attention, while the remaining 120 pages of the volume are assigned to concise descriptions of each important metal, its occurrence, history, preparations, properties, and most common or characteristic salts. The information contained in this volume is, of course, to be got, and got easily, elsewhere; for instance, in such a textbook of chemistry as Fownes' or Roscoe's. In fact, it is rather difficult at first sight to see why the present compilation was made. Mr. Wood explains in his preface his chief reason for publishing these notes on the metals; it is the first book on chemistry which has been written of late years without the introduction into it of the word *acid*. This word has been used, doubtless, freely and promiscuously—we may add, improperly; but to banish it from chemical nomenclature on this account is weak and, we think, quite unnecessary. Our author has written this book, then, that he may suppress the word altogether. One would almost think that there was something indecent about the word *acid*, so anxious is Mr. Wood to bury it out of sight! Now, we maintain that *acid* is a useful term, that it is justly applied to certain hydrogen compounds, both organic and inorganic, and that the disuse of the term leads to a loss of precision, and to the coining of less expressive words (such as “non-basyloous,” p. 27). But, even if we accept Mr. Wood's arguments on this point, we cannot think the execution of the work satisfactory in other particulars. We cite a few passages which demand improvement:

On page 48 we are told that nitre is used to effect oxidations, because it “easily eliminates oxygen;” surely it would be better to say “evolves” or “furnishes oxygen.” On page 49 potassium chloride is said to occur “in beds mixed with common salt;” this statement conveys an erroneous notion as to the mode of occurrence of sylvine or carnallite, the chief minerals containing the salt mentioned above. On page 51 potassium cyanide is said to smell like essence of bitter almonds; of course it smells like prussic acid; but to identify the odours of prussic acid and the bitter almond is to endorse a ludicrous error started by the ‘Pall Mall Gazette’ in a recent poisoning case. It was there affirmed that almond-flavouring owed its pleasant taste entirely to the prussic acid present in it. On page 52 we find the formula KS for a potassium sulphide. Is there such a compound? At page 54 we are told that the metals, potassium and sodium, commonly occur together in the same substance, and that the metal “sodium is found native in especially large quantities.” Gold occurs native, so does platinum, but we always thought that sodium was first obtained as a metal by Sir Humphry Davy in 1807. When a metal is spoken of as found native, the

expression has but one meaning, that the metal occurs uncombined. No subsequent qualification of the word can be admitted, as at p. 54. The chemistry of the phosphates on page 83 is at fault. We are told that bones, guano, &c., "are decomposed by H_2SO_4 , and converted into *superphosphate of lime*, which, being soluble in water, can easily be absorbed by the growing plant, and are (*sic*) especially adapted for turnips." Every agricultural chemist will perceive the inaccuracies of this statement; every mineralogist will likewise decline to allow Mr. Wood's next assertion, that tricalcium phosphate occurs as a natural crystallized mineral, *apatite*. With every 5 atoms of calcium and 3 of phosphorus in *apatite* there is always combined 1 atom of chlorine or fluorine.

Principles of Chemistry.¹

It would be impossible in a brief notice to do justice either to the original work of M. Naquet or to this excellent translation of it into English. The book embraces the whole domain of descriptive chemistry, yet less attention is paid in it to the minute cataloguing of the properties of a substance, be it mineral or organic, than to the study of its relationships, modes of formations and probably constitution. The original treatise shows signs throughout of conscientious and unremitting labour, the newest researches—English, German, and French—being duly set in their places, whenever the results obtained have thrown light upon theories of chemical construction. The translator's work has been successfully accomplished, and we find that the meaning of the author is usually conveyed in language at once easy and accurate. The illustrations and diagrams introduced into the text of M. Naquet's work are given also in the English edition; they are numerous and instructive. One chapter in the work we feel bound to single out for particular commendation: it is the last in the volume, and is devoted to the history of synthesis in organic chemistry.

Kennion's Harrogate.²

THE name of Dr. Kennion is intimately associated with Harrogate,

¹ *Principles of Chemistry founded on Modern Theories.* By A. NAQUET. Translated from the Second Edition by WILLIAM CORTIS, and revised by THOMAS STEVENSON, M.D. London, 1868. Pp. xxviii, 848.

² *Dr. Kennion's Observations on the Medicinal Springs of Harrogate.* Seventh Edition. Enlarged and revised by ADAM BEALEY, M.D., &c. London, 1869. Pp. 56.

where his professional talent and gentlemanly bearing secured him the foremost position as a consulting physician, and the esteem of his fellow-townsmen, who have signified their appreciation of his worth by placing his portrait in their principal public building. Since his decease, Dr. Bealey has taken up his residence in the town, and has put forth anew the little book Dr. Kennion first published in 1853, explanatory of the uses of the mineral waters of the locality. This small treatise had the merit of not being a mere advertisement of the place and of the writer, as so many 'Guides to Mineral Waters' frequently are. It presented a sufficiently full account of the composition and qualities of the waters, and indications of the class of disorders for which those waters might fairly be looked for to benefit. Dr. Bealey, in re-editing it, has shown judgment in not meddling with the text, but has kept separate his own contributions in the shape of a preface and of an introduction. We are, however, glad to find a complete harmony pervading the views advanced both by author and by editor. Dr. Bealey attempts no excessive laudation of Harrogate or of its waters, but points out under what circumstances of disorder recourse to such watering places may be had with advantage, and also the class of cases for which Harrogate waters are particularly beneficial. These sulphurous-saline springs are sufficiently well known, and the large numbers of invalids who yearly resort to Harrogate prove them to be generally valued; but we would recommend medical men at large to make themselves more fully acquainted with their composition and accredited properties, by possessing themselves of this inexpensive little treatise. And it is here worth while to remark, that not only have we at Harrogate the most potent sulphur waters, but also a unique chalybeate containing a chloride of iron.

Dalton on Physiology and Hygiene.¹

"THIS book is intended as a means of instruction in physiology and hygiene for pupils and general readers," so the author states in his brief preface, and from a general survey we have made of its contents, it is very well adapted for its purpose.

It is written by Dr. Dalton, Professor of Physiology in the College of Physicians and Surgeons, New York, a circumstance that gives a further guarantee of the correctness and value of the information conveyed in its pages. It has, moreover, found an English

¹ *A Treatise on Physiology and Hygiene for Schools, Families and Colleges.* By J. C. DALTON, M.D. With Illustrations. London, 1868. Pp. 399.

publisher in the well-known firm of Sampson Low and Co., who have brought it out in a very readable form, and sufficiently well illustrated it by wood-cuts that will really assist the reader in comprehending the text.

Great pains have been taken by the author to avoid complicated names and phrases, and where technical words are employed their meaning is explained. To farther facilitate, moreover, the interpretation of such words a glossary is appended to the book giving short definitions, and in many instances their etymology. At the end of each chapter an array of questions is introduced, whereby to test the learner's acquisition of the facts detailed.

The eighteen chapters into which the work is divided are severally devoted to recording the facts and generally received doctrines respecting the mechanism and functions of the human frame, and the character, properties and uses of food.

Such is an outline of the nature of the contents of this popular treatise. We will end our notice of it by saying it may safely be put into the hands of any readers who desire to learn something of themselves, the modes and laws of their existence.

Mortality among Parturients.¹

IN the present day, when attention has been so thoroughly directed to child-bed mortality, reliable statistics on the subject cannot fail to be interesting to the medical reader. Those quoted in the author's paper are derived from the medical reports sent in to the Department for the Interior. It is to be regretted that the mortality from puerperal fever is not directly exhibited in these reports, deaths from all causes connected with the act of parturition being classed together under one common head. As puerperal fever is, however, the most frequent cause of such deaths, there can be no doubt that the varying mortality in the several districts will indicate the localities in which that scourge has more or less extensively prevailed. From the following Table it will appear that during the five years the puerperal mortality was very different in the five dioceses therein specified:

¹ *Om Dødeligheden blandt Barselkoner i Norge i Femaaret, 1861 til 1865.* Af C. EGER. ('Norsk Magazin for Lægevidenskaben,' xxiii Bind, 6 Hefte.) 1869.

On the Mortality among Parturient Women in Norway in the Quinquennial Period, 1861—1865. By C. EGER. Published in the 'Norwegian Magazine of Medical Science,' Vol. xxiii, Part 6.

In the five years, 1861—1865.	Diocese of Christiania.	Diocese of Christiansand.	Diocese of Bergen.	Diocese of Thronhjem.	Diocese of Tromsö.
Number of living and still-born children	114,465	52,107	42,278	37,639	27,532
Deduct for twins and triplets	1,488	570	553	442	348
Number of parturient women	112,977	51,537	41,725	37,197	27,184
Deaths of parturient women .	570	399	330	289	314
Proportion of deaths to parturient females	1 : 198	1 : 129	1 : 126	1 : 128	1 : 87
Being in the proportion for the whole kingdom of 1 : 138.					

The cases of triplets for the five years were 47, or one in 5757. The twin cases were 3307, or 1 in 83.

While the diocese of Christiania appears, from the foregoing Table, to be comparatively favorably circumstanced, the mortality in the city itself is 1 : 118, being exceeded, in a list of eighteen districts, only by Finmark, where it is 1 : 73 ; Nordland, where it is 1 : 106 ; and Stavanger, where it is 1 : 115. In this list Christians Amt has the smallest mortality, 1 : 293.

The author, in commenting upon his statistics, observes that—

“There are consequently circumstances in Christiania which cause parturient women, notwithstanding the most diligent care, and speedy and able medical help, to be carried off by death in greater number than in any other part of Southern Norway.”

And he subsequently adds that—

“All which can at present be inferred from the foregoing statistical data concerning the greater mortality in Christiania, without a knowledge of the proximate causes, is, that a pregnant woman by being confined at Hedemarken, Toten, Gudbrandsdalen, or some place in the upland districts, will have a prospect of escaping with her life, twice as favorable as if she were delivered in Christiania ; and that this will be the case, especially during the prevalence of an epidemic of puerperal fever, may safely be asserted.”

The same, he observes, will probably prove to be true of many other towns, though the statistics from these are on so small a scale that it would not be advisable to draw from them any definite conclusion. He has, however, no hesitation in asserting that the mortality among parturient women is, without reference to the cause, much less in the inland districts of the country than in the littoral regions, Christiania, and in Finmark, a statement which may be received as the rule until subsequent and more accurate reports either modify or overthrow it.

Sir W. Jenner's Addresses.¹

THE profession will be glad to have these two most able addresses by Sir W. Jenner, as now republished, well printed, and bound together in a convenient form. They are so well known, having been brought out by the several principal medical journals, that an account of their contents is uncalled for. Yet we must remark, that no one can read them without much profit or without a sense of satisfaction that the advance of medicine, both as a science and an art, has of late years been so decided, and that future progress is so promising. The title is happily chosen. The distinguished lecturer shows to what point we have reached, marks the substructure upon which future work must be based; and, particularly in the address to the Epidemiological Society, points to the relations subsisting between medicine and the State, and the necessity for legislation and State supervision to cope with certain diseases to make medicine a success.

Fotherby's Oration.²

THE custom of scientific societies usually demands an annual oration, in which the merits of the society discoursed upon require to be duly set forth. Such customary orations are apt to degenerate into vapid talk, and are allowed to vanish into thin air. But now and then the orator of the occasion finds a subject to give body and countenance to his address, and secure for it at least the ephemeral existence that commonly falls to the lot of pamphlets.

The address before us is one of those that have a subject to give them a "*raison d'être*," which in this instance is the history of the Hunterian Society of London; the fiftieth anniversary of which Dr. Fotherby had to celebrate. It has been published at the request of the Council, and will be particularly valued by present and future members of the Society.

It is well and elegantly written, and, besides the history of the Society, gives a good sketch of the rise of scientific associations, and dilates on their usefulness, particularly those instituted for the

¹ *The Practical Medicine of To-day*. Two Addresses delivered before the British Medical Association and the Epidemiological Society. By Sir W. JENNER, Bart., M.D., F.R.S., &c. &c. London, 1869. Pp. 59.

² *Scientific Associations; their Rise, Progress, and Influence; with a History of the Hunterian Society*. An Oration. By HENRY J. FOTHERBY, M.D., &c. London, 1869. Pp. 59.

advancement of medicine and the promotion of the interests of the medical profession.

Chavasse's Counsel to a Mother, and Baines on Infant Mortality.¹

WE group these productions together as there is this much in common between them, that they aim to open the eyes of the public to the prevailing disregard of the laws of health, and to point out the direction to be taken to save infant life, and to promote the perpetuity, health, and vigour of our race. Their subject-matter is, therefore, of the greatest importance, and deserves to be fully discussed, but as we propose very shortly to take up the question of infant mortality at some length, we shall at present content ourselves with briefly indicating the scope of the treatises now under notice.

Mr. Chavasse has already acquired much deserved popularity by his little book entitled 'Advice to a Mother.' The one just published is to be regarded as a supplement to it. "The two works (says the author) ought to be inseparable, and should be considered as a work complete in two volumes." He divides his book into three parts, headed severally Infancy, Childhood, and Youth, and proceeds to lay down rules for the nursing, tending, and management of those belonging to each such age. This is done in a conversational manner. The anxious parent is supposed to put a string of pertinent questions, to which Mr. Chavasse replies at greater or less length, and succeeds in introducing into his answers a mass of valuable information.

It is not a book addressed to medical men—not but that many of us might pick up useful hints even from this volume of Churchill's 'Popular Medical Series,' particularly as the author informs us that he has spent thirty years of his life in special study of the art of treating and managing children. It is written for ladies, and the writer is evidently impressed with the desirableness of a light, playful style, ornate with scraps of poetry and witticisms. Indeed, poetry so abounds that Mr. Chavasse feels himself called upon to apologise for its frequency; this he does by assuring his readers that he is brimful of poetry, and to illustrate this submits to "an impartial public" a poem on childhood, now first published in the present volume. Bearing in mind the golden maxim "*ne sutor ultra crepi-*

¹ *Counsel to a Mother.* Being a Continuation and the Completion of 'Advice to a Mother.' By PYE HENRY CHAVASSE. London. Pp. 180.—*Excessive Infant Mortality; how it can be Stayed.* To which is added a short paper on 'Infant Alimentation.' By M. A. BAINES.—*On the Prevention of Excessive Infant Mortality.* By MRS. BAINES.

dam'' we venture not upon its criticism, yet we are convinced no mamma can read it without commending the poet for truthfulness in the delineation of childhood's ways.

But apart from its trappings of poetry and small talk, it is full of good counsel to mothers, and will well sustain the author's position as a popular teacher of medical and physiological truths.

The inquiring mother is supposed to have learnt the fact of the enormous mortality of infants in this country, and to seek an explanation. This is vouchsafed by Mr. Chavasse by a quotation from Miss Martineau that exactly met his requirements. The laws of health are neglected by mothers; the natural course of things is interfered with; Nature provides their first food, but of this infants are too frequently deprived by the vanity, idleness, mistaken notions or greed of their mothers; it rarely happens from inability to suckle their offspring.

These, too, are the great truths which Mrs. Baines has been so praiseworthily active in enforcing upon the too unwilling minds of the women of England. She has sought to arouse their sympathies by revealing to them the terrible destruction of infant life, and to enlist their aid in lessening it, by showing it to be in a great degree preventable and how to prevent it.

Proper infant alimentation is her first recommendation, and as a matter of course this consists in the maternal suckling of the child. Where this plan cannot be followed she prefers, on the whole, artificial feeding with cow's milk, mixed with a small portion of farinaceous substance, to nursing by a hired wet nurse. In this point she differs from Mr. Chavasse and medical men generally; but the divergence in recommendations between herself and that gentleman becomes very pronounced when the latter suggests an unmarried woman as a more fitting wet nurse than a married woman. This is a proceeding on which Mrs. Baines has thought much and written much, and one which she cannot too strongly condemn as opposed to morality and social happiness. In the condemnation of this proceeding, moreover, Mrs. Baines has had the concurrence of most of our social reformers, and although Mr. Chavasse has apologies for it, and, indeed, arguments in its favour, it would be well for him to reconsider his counsel on this matter in a future edition of his little book.

We will not proceed further with an examination of Mrs. Baines's views, hoping to return shortly to their consideration. But before dismissing this notice of her papers we owe her an apology for the delay in its appearance.

Alvarenga on the Foramen Ovale.¹

WE have before us a pamphlet showing very close attention to, and an intimate acquaintance with, the subject it treats of, gained both from the works of others, and from personal observation.

It commences with an excellent history of the investigation of this subject from the earliest periods to the present time, and finishes with the conclusions drawn from the author's own special researches.

From a study of the works of Galen, Carcanus, Harvey, Mery, Morgagni, Billard, Longet, and many others, Dr. Alvarenga concludes that the subject has not been sufficiently attended to, though from these authors some knowledge of several important facts has been gained, the chief of which are—

1. That the foramen ovale and arterial canal close completely after birth, or remain open for a longer or shorter period—until maturity, or even until old age—but that the foramen ovale in adults is smaller than in the fœtus.

2. That from the presence of the foramen in the adult we are not bound to suppose that the blood of the two auricles mixes, since the presence of the valve may prevent that.

3. That by induction we may conclude that the closing of the foramen ovale takes place at some period or other of life, but that the longer it exists the more rarely it takes place, and the more difficult it is of accomplishment.

Dr. Alvarenga determined to fully investigate the subject himself, hoping to arrive at more definite results. For this purpose, assisted by M. Almado, Curator of the School of Medicine and Surgery at Lisbon, he made an examination of the hearts of all children who died at the Hospital of St. Joseph, and at the Foundling Hospital, Lisbon, entering the particulars of each case in tables drawn up for the purpose. These tables contained—

The number in order of time, and the date of each observation :—the age, sex, and total weight of each infant :—the weight of the heart, of each lung, of each kidney, of the liver, of the spleen, of the encephalon, and of the cerebrum and cerebellum separately ;—and lastly, a statement of the absence or presence of the foramen ovale and arterial canal in each case.

We will now give the results of Dr. Alvarenga's investigations, and see how far they advance our knowledge of the subject.

¹ *Considerations et Observations sur l'époque de l'occlusion du trou ovale et du canal artériel.* Par Dr. PEDRO F. DA COSTA ALVARENGA. (Pamphlet.) Lisbon, 1869.

There are three sets of tables. The first set gives the results of the observations with regard to the time of closing of the foramen ovale. The second set gives the results of these respecting the obliteration of the arterial canal. The third, the results of a comparison of the relative times of closing of both these structures in the same individual.

Of 213 hearts examined, of subjects of various ages between one day and twelve years, to determine the first question, 193 were of subjects under six months of age; and in only two instances in these 193 cases was the foramen ovale found closed. One of these cases was that of an infant two and a half months old, the other of one three months old.

The remaining twenty cases were of subjects of various ages between six months and twelve years, and in only six of these twenty cases was the foramen ovale found closed.

In the tables exhibiting the results of the researches in reference to the state of the arterial canal, of 130 examined 72 were of subjects under thirty days old, and none of these 72 had the arterial canal closed.

Of the remainder, 49 were between thirty days and four months old. In 19 of these the canal was obliterated, the youngest being thirty days old.

The rest (9) were of various ages between four months and twelve years, and 6 of them had the arterial canal closed; one of the exceptions being a child four and a half years old.

In our opinion Dr. Alvarenga has by no means, as yet, satisfactorily settled the question. It seems to us that a much larger number of examinations must be made on subjects between six months and twelve years of age, before we can be certain of even the approximate age at which obliteration of the foramen ovale normally takes place.

We have seen that out of 213 cases only 20 were of subjects of more than six months of age, and of these only 6 had the foramen closed. One of these six was a subject twelve months old, three were between twelve and nineteen months, the fifth was two years old, and the youngest six months.

Hence it would appear that the proportion of cases between the ages of six months and twelve years, in which the foramen ovale is closed, is only 30·00 per cent. But twenty cases are not sufficient to draw a satisfactory conclusion from.

On the other hand, as in 193 cases of subjects under six months old only two had the foramen closed, or 00·96 per cent., we may satisfactorily conclude that obliteration of this structure is very rare indeed before six months of age.

Again, with regard to the closing of the arterial canal:—

Out of 130 cases examined, only 33 were of subjects over two

months old, and 15 of these had the canal obliterated, or nearly 50·00 per cent. The remaining 97 cases were of subjects between one day and two months old. Only one of these had the canal obliterated (an infant thirty days old) so that we may infer from this that the arterial canal is very seldom obliterated before the sixtieth day.

But as in 15 cases out of 33 of subjects over two months and under twelve years old, the canal was obliterated, we may conclude that that structure is usually entirely obliterated by the twelfth year, for only one of the cases was of a subject over four and a half years old; but here, again, the cases are too few in number for a satisfactory conclusion.

To sum up—can we fairly deduce from these results more than this?

1. That the foramen ovale never closes before two and a half months, and only very rarely indeed before the age of six months, and that it frequently is found open up to the twelfth year.

2. That the arterial canal is never obliterated before the thirtieth day, and very rarely before the sixtieth, and that it is usually closed before the end of the fifth year.

From the third set of tables containing comparisons of the relative times of closing of both these structures in the same individual, we draw the following conclusion:—

1. That the arterial canal is usually obliterated before the foramen ovale is closed; for out of 130 cases, in 15 of which the canal was obliterated, only 5 had the foramen ovale closed. But in these 5 cases 2 had the arterial canal open, so that in 15 cases in which the canal was closed only 3 had both structures obliterated.

More, we think, we cannot justly conclude from the results noted down in these tables, especially on account of the small number of cases over six months old examined with regard to the state of the foramen ovale.

Much remains to be cleared up concerning the condition of the foramen ovale between that period and the twelfth year, before we can have any accurate knowledge of the time of closing of this structure.

However, Dr. Alvarenga states that he is going to continue his investigations, and promises another pamphlet containing further results.

Surgery of the Mouth.¹

WE have classed these two pamphlets together because they both have reference to the mouth; the one to its deformities, the other to its injuries. Messrs. Ramsay and Coles have dealt with the congenital and accidental defects of the palate; while Mr. H. O. Thomas has given us a valuable suggestion as to the means of treating fractures of the jaw.

Dentistry has made great and encouraging progress of late years. We owe it no small thanks for the comfort we enjoy—indeed, some would tell us that it had done much to prolong human life, by enabling elderly people to masticate their food better than they could with such teeth, or stumps of teeth, as nature had left them. However this may be, there is no doubt that dental art has advanced rapidly, and that a happy combination of dentistry and surgery enables us to deal with cases which half a century ago were well nigh hopeless. This is true of the injuries of the jaws, and it is true also of the congenital deformities of the mouth. Let anyone turn to Mr. Christopher Heath's Jacksonian Prize Essay, and he will see how much may be done in the worst cases of fractures of the jaw when dentistry and surgery go hand in hand. And the little work before us by Messrs. Ramsay and Coles shows how much may be done in the severest cases of cleft palate when the surgeon avails himself of the skill of the dentist.

The authors of this thin volume do not discuss the operative treatment of cleft palate, except so far as it is necessary to their purpose to do so. That they leave to the surgeon. Their proper subject is the mechanical treatment of the deformities of the mouth; and in this department their success appears to have been so great that they are doing the profession a service by publishing their results.

Though the operations for cleft palate have been brought to a wonderful degree of perfection, the results are not so satisfactory as we could wish. We have rarely seen a case, however creditable it might be to the operator, in which the patient obtained anything like a full and perfect articulation. Perhaps this may be because surgeons have hitherto thought that it was undesirable to interfere before the age of puberty; and now that some are practising the operation at an earlier age we may see happier results. Whether this should be the case or not, there will always, we fear, be mal-

¹ 1. *The Mechanical Treatment of Deformities of the Mouth, Congenital and Accidental.* By ROBERT RAMSAY and JAMES OAKLEY COLES, Members of the Odontological Society. London, 1868. Pp. 95.

2. *Cases in Surgery Illustrative of the New Method in Applying the Wire Ligature in Compound Fractures of the Lower Jaw.* By HUGH OWEN THOMAS, M.R.C.S.L. London, 1869. Pp. 15.

formations of the palate so severe that surgery will be unable to deal with them effectually, and in these instances we must seek the aid of mechanical appliances. It is to this class of cases that Messrs. Ramsay and Coles have paid special attention, and the present work contains an account of their method and a *résumé* of their experience.

For an explanation of their various modes of procedure, and of the materials they employ, we must refer our readers to the volume itself; but we may here quote, in an abbreviated form, one or two cases which show what can be done by the skilful and scientific application of a false palate:

“Mr. D., aged 19, cleft of hard and soft palate, extending also through the dental arch, a single harelip having been treated in a very satisfactory manner soon after birth. The left side of the maxillary portion of the fissure at its border was continuous with the vomer, thus giving no overlap, except on one side and at the apex of the cleft. In these cases the incisors are generally but very imperfectly developed; we, therefore removed the centrals and considerably improved the mouth for the purposes of speech and mastication, by fitting artificial teeth in their place on the front piece of the velum. The artificial palate was then made. This was put in, November, 1865, and has been worn with great comfort ever since. In less than a twelvemonth the articulation of every word and sound was perfect; and those most difficult letters, K and R, were pronounced with the greatest ease and precision. The only thing that could not be overcome was the peculiar nasal tone that results always from the malformation that occurs in the nose after the operation for harelip. Before the patient was put under treatment, his speech was quite unintelligible, even to many of his friends; yet, after the mouth was restored to as symmetrical a form as, under the circumstances, was possible, this gentleman, by his own perseverance, unassisted by anyone’s tuition, acquired such clearness of utterance as to elicit expressions of the greatest astonishment at both the Medical and Chirurgical and Odontological Societies, before whose members he very kindly went through the alphabet and several difficult sentences, and within a short time of this he was elected an officer in a volunteer rifle corps, as well as a volunteer fire brigade, thus affording the best evidence of the ability he possessed to make himself readily and perfectly understood” (p. 76).

Here is another case, of a somewhat similar kind, in which the successful result is attested by equally satisfactory evidence:

“Mr. R., æt. 22, cleft of the soft palate, extending just beyond the posterior margin of the hard palate, treated May, 1864. The voice in this case was very bad. The young gentleman was most anxious to have something done, as a public appointment was being kept for him, provided his speech could be rendered intelligible. A velum was made fitting into the cleft; but, unlike the others shown,

it had only one flap at the posterior part, the two sides of the cleft embracing it only to the commencement of the bulbous portion of the bifurcated uvula. Though this extremely simple form of instrument was used, the result was such that in two months he was able to enter upon the duties of the appointment that had been held open for him " (p. 82).

Let us now turn to the second of the two pamphlets which we have named at the head of this notice. In this essay Mr. H. O. Thomas explains the way in which he has tied together the fragments of the jaw in cases of compound fracture. His method consists in passing a wire through both fragments, and then coiling it upon itself at each extremity, so as to form a sort of button. This button at each end of the wire prevents it from slipping, and, if it requires to be tightened, it is a simple matter to make another turn on the coil at either end or at each end.

It is not easy to give a clear idea of the proposed plan in words; but a glance at the woodcuts with which the account is illustrated will explain it in a moment, and will show also the instruments which Mr. Thomas has devised to enable the surgeon to carry out his suggestion. Several cases are related in which the author has used this plan with success; and it is one which is worthy of the attention of surgeons, for it promises to be a valuable addition to our means of dealing with a very troublesome class of cases.

Life of Benjamin Bell.¹

HOWEVER much the life of Benjamin Bell might have been worth writing when his name and fame were fresh in the memory of his professional countrymen, we fear that the biographer who now undertakes to record it, will arouse but limited interest among a generation living nearly a hundred years since Bell held a leading position as a surgeon and a writer on surgery;—a generation that has become the inheritor of the labours of a series of surgeons who have followed him, equally skilled in their art, and at least equally matched with him as teachers of that art. The complacency that must attend a sacrifice offered to the shades of a departed ancestor, and the attendant gratification diffused among the members of the family circle and kindred will, in our apprehension, constitute the only reward the worthy writer of the biography can obtain,—not that we hold in light esteem the good work contributed towards the development of modern surgery by Benjamin Bell, *primus*; on the contrary, in a history of surgery his name must occupy no inconspicuous place. Nor again, is anything but com-

¹ *The Life, Character, and Writings of Benjamin Bell, F.R.C.S.E., &c.* By his grandson, BENJAMIN BELL, F.R.C.S.E., &c. Edinburgh, 1868. Pp. 170.

mentation to be written respecting the manner in which that eminent surgeon's living name-sake has performed his task. Yet withal this memoir comes upon us as a thing out of date, and the impression will obtrude itself that the sketches of the life of Bell in Kay's 'Edinburgh Portraits,' in Chambers' 'Lives of Distinguished Scotchmen,' and in Anderson's 'Scottish Nation,' alluded to by the writer, might be accepted as furnishing sufficient memorials of the man. The principal inducement to undertake the biography, as stated by the author, was the discovery of eighty-two letters addressed by Benjamin Bell to his father and mother, between the years 1767 and 1793. "They are mainly occupied with family concerns, and therefore do not admit of being largely quoted," and, therefore (if we desired to be critical, we should write) did not call for a memoir to reproduce them. But the author goes on to say that by them "our acquaintance with the character and dispositions of the writer is rendered full and satisfactory;" but here again we are compelled to remark that at this day the private character and dispositions of the old surgeon are of the smallest possible interest to the present race of medical men or to the public. His niche in the temple of fame was earned by his work for the advancement of surgical knowledge, and it is only the history of that work that will claim the attention of the present and future generations.

His life, moreover, was an uneventful one. He was born in 1749, and died in 1806; was a pupil at Edinburgh when the University was at the height of its greatness, enjoying the reputation of the Monros, of Black, Cullen, Rutherford, and the Gregorys; became a dresser, and soon a surgeon's clerk, and in his twenty-fourth year a surgeon of the Infirmary. From that time his talents and character secured him a prosperous professional career, marred indeed to a considerable extent by bad health and early infirmity. The system of surgery, which passed through seven editions under his own hands, and became the text-book of the day, constitutes his most enduring monument. His grandson and biographer wrote a review of his contributions to surgery in the 'Edinburgh Medical Journal,' for November, 1868, and has reprinted it as an appendix to the present volume. From an historical point of view this notice and estimate of Bell's professional writings will possess a value to the inquirer into the modes of practice pursued at a long-past period, and into the successive stages whereby modern surgery has been built up.

Squire on Temperature.¹

MR. SQUIRE has done well to reprint this instructive paper read before the Obstetrical Society of London. It contains the results of much diligent personal observation, and constitutes an important chapter in the history of temperature in relation to the body in health and in disease. It demands no analysis here, but we hope soon to be able to offer a digest of the observations it records in connection with a survey of the whole subject of temperature of the body in its medical aspects.

Hogg on Cataract.²

THIS is a well-arranged lecture, impressing on ophthalmic surgeons the desirability of further investigating the cause of cataract, inasmuch as the author hopes that "the time is not far distant when this change in the nutrition of the lens will be perfectly under the control of therapeutical agents."

The case of Sir David Brewster, and his propositions for the cure of cataract, are alluded to; likewise those of Sperino, who tried paracentesis extensively; but, as the author remarks at the commencement of his lecture, "What can be said about cataract and its treatment which has not been repeated over and over again?" Indeed, this lecture is but a résumé of the experiments of others, and we fail to notice any original ideas or suggestions advanced by the writer that will be of much service in the attempt to arrive at the object it sets forward as requiring elucidation.

Cotton on Phthisis.³

THE appearance of a fourth edition of this small special treatise on the physical signs of consumption is, at the same time, evidence of its want and of its value being fully appreciated by the members of the profession. The character and teachings of the book have, in past times, been sufficiently set forth in this 'Review,' and no one is better qualified by experience than its author to give instruction in the subject it treats of. It is enough here to call attention to the

¹ *Infantile Temperatures in Health and Disease.* By WILLIAM SQUIRE, L.R.C.P. Lond. London, 1869. Pp. 29.

² *Cataract and its Treatment.* By JABEZ HOGG, F.L.S. (Pamphlet.) 1869.

³ *Phthisis and the Stethoscope; or, the Physical Signs of Consumption.* By R. PAYNE COTTON, M.D. Fourth Edition. London, 1869. Pp. 112.

publication of this new edition, and to state that it has undergone a careful revision; such alterations and additions having been made as further experience and observation appeared to justify.

On Diaphoresis.¹

WE do not comprehend for whose information this pamphlet is published. To medical men its revelations are "as old as the hills;" to the public it lacks the matter to make it popular. Most of us possess the rudimentary knowledge about the skin, its pores and perspiration, and the exhalation of carbonic acid, with which one half of the pages of the brochure is occupied; and the benefits of active cutaneous transpiration next enlarged upon, we may venture to say, are about as generally recognised. Yet, singularly enough, the writer imagines that he is announcing a new discovery in extolling diaphoresis as a remedy in fever and inflammation. But not to misrepresent him, let us add that he looks to diaphoresis as a cure in other morbid states. A case of delirium tremens got well in less than a week, but it was the only case he had to treat. A solitary case of ague that fell to his lot in practice also got well in some six or eight days. A patient with deep mental, domestic shock, was sweated out of it; and if, as the author tells us, he got a chance of treating a case of hydrophobia, tetanus, or cholera, diaphoresis would be the panacea.

Such being the wide and potent virtues of good sweating, he triumphantly asks, "How is it to be done?" He longs to unburden himself of the secret; he struggles through another page in bringing it forth, recounting the "much thought, reading and observation in this country, on the Continent, and in the southern regions of the world," that its development has demanded of him. But to tantalise the reader no longer, here it is: "Spirit. Æth. Nit., Vin. Ipecac., Ant. Pot. Tart."—tria juncta in uno, with a threefold force that no solitary disease can or ought to withstand. This is the very kernel itself, though enshrouded in sixteen pages of print.

Winn on Hereditary Disease.²

IN the course of his experience with mental disease, the author, like all other observers in the same field, has noticed that, if in one

¹ *Diaphoresis; a Powerful Aid in the Arrest and Removal of Human Disease, and thereby Prolonging Life.* By CHARLES CLARK, M.A. Cantab., M.R.C.S.E. London, 1869. Pp. 16.

² *On the Nature and Treatment of Hereditary Disease with reference to a Correlation of the Morbific Forces.* By J. M. WINN, M.D., &c. London, 1869. Pp. 31.

member of a family insanity has developed itself, some other disease of a hereditary nature often has manifested itself among the rest; and also that, in the same individual, insanity may apparently replace another malady, or in its turn give place to one. "These facts (he says) forcibly suggest a correlation of morbid forces." "In like manner (he continues) the history of zymotic diseases, which, while presenting marked special differences, yet are subject to the same general laws, is suggestive of a similarity of morbid force."

The author next proceeds to argue that vital force (to which he assigns an individuality) is correlated with other and physical forces, and expresses his conviction that "one and the same latent force obtains in all who are predisposed to the various hereditary diseases," a force that may be called "morbid," and which may lie dormant until developed by some exciting cause. He further presumes that "a deleterious power may be inherent in the molecules of the nervous system" in instances of so-called hereditary predisposition.

This hypothesis he pronounces more plausible than that of the presence of a *materies morbi* in the blood, and forthwith falls foul of Dr. Todd on account of his arguments against solidism. In so doing, he makes Dr. Todd talk great nonsense by misinterpreting the simple statement of that eminent physician and physiologist, respecting the phenomena of impregnation, and thereupon proceeds to demolish the fiction he has himself set up:—this done, he rests contented with the supposition that he has established the doctrine of a morbid force inherent in the molecules of the nervous system, constituting the cause of the predisposition to hereditary disease. But, in our apprehension, the whole course of reasoning employed to establish this hypothesis is parallel to that which some of the older philosophers hit upon to prove the existence of "rays of cold." A negation of force being a positive force, is a conception that has not yet reached us.

The remainder of the brochure is occupied with miscellaneous remarks on hereditary disease, the author taking occasion to give advice to those about to marry; and, once again, referring to the "one morbid cause" as an established truth, arrives at the satisfactory conclusion that there is "only *one* general plan of treatment for the whole" of the "extensive class of hereditary diseases." This *one* general plan of treatment, however, resolves itself into attention to hygiene at all ages, and the selection of appropriate occupation and a fitting climate; in short, into means of invigorating the health of mind and body, such as experience has taught mankind from the earliest ages, and which are not the peculiar logical deductions from the hypothesis of a morbid force potentialised by disturbed nerve-molecules.

Pharmacopoea Suecica.¹

THE seventh edition of the 'Swedish Pharmacopœia,' just issued from the press, has reached us in the form of a neat, clearly-printed, and very portable volume, which strikes us as being the model of what a pharmacopœia ought to be: comprehensive, accurate, and free from all redundancy. We shall briefly describe its leading features.

The work is published in Latin. This adherence to a plan now becoming obsolete in countries as England and France, whose languages are widely spoken and still more extensively read, is perhaps due partly to the restriction of the Swedish within narrow limits, partly to the wish which seems to be implied in the preface, that a common pharmacopœia may, ere long, be adopted in Sweden, Norway and Denmark. Under other circumstances the use of the vernacular tongue would probably be preferable.

For the first time the metrical system of measures and weights is introduced into a Swedish pharmacopœia. It may be useful to some of our readers to know that the *mètre* is equal to 3·36806 Swedish feet, and that the equivalent of the Swedish pound is 425·0758 grammes. But as it is possible that in some prescriptions the old system may for a time be adhered to, two tables are appended to the work exhibiting the relations of the Swedish commercial, and of the old Swedish medical weights, to the French grammes, centigrammes and milligrammes.

The preparation of the pharmacopœia was intrusted by the Royal College of Health to Professor P. H. Malmsten; Drs. O. T. Sandahl, and N. P. Hamberg; and W. Anderberg and J. J. Braconier, pharmacopolists, under the chairmanship of the President of the College. With a view to ensure as much correspondence as possible between the pharmacopœias of Sweden, Norway and Denmark, this committee was required by a Royal edict of the year 1861 to consult not only with the Swedish Society of Physicians and other Swedish physicians and pharmacopolists, but also with delegates specially chosen from Norway and Denmark.

In the arrangement the plan adopted in the *Pharmacopoea Borussica*, 1846, the *Adumbratio Pharmacopoeæ Austriacæ*, 1852, the *Pharmacopoea Norvegica*, 1854, and in the *British Pharmacopœia*, 1867, of including the *Materia Medica*, and the preparations and compounds in a single series in alphabetical order, has been followed. This contrasts favorably with the system still maintained in the

¹ *Pharmacopoea Suecica*. Editio Septima. Stockholmæ, 1869. Typogr. Reg. The Swedish Pharmacopœia. Seventh Edition. Stockholm, 1869. 8vo. Pp. 275.

French Codex Medicamentarius, where the pharmacopœial part proper alone is divided into no less than seventy-five chapters!

In addition to the Tables we have already mentioned is one exhibiting the maximal doses of active medicines admissible for internal administration to adults. Prescriptions in which these doses are exceeded are not to be dispensed unless the excessive dose be marked by the prescriber with a note of admiration, or with the word "sic."

Liquid medicines are to be dispensed by weight, not by measure.

Material changes of nomenclature are, as far as possible, avoided. A table of chemical reagents is appended.

Throughout the work signs are used indicating those medicines which are not to be dispensed without a physician's prescription, and which are to be kept with those similarly marked, separately from all others. Medicines marked with a double sign are to be kept under lock and key. Drugs not in ordinary use are marked with an asterisk, and need not be kept in the pharmacy unless by the previous direction of the physician. Lists of these three classes of medicines are given at the end of the volume.

Our space does not admit of a detailed examination of the preparations. They appear to be characterised in general by simplicity and utility. A form for an "Acetum thebaicum" replaces that for the "Liquor sedativus." We have an "Aqua Amygdalarum amararum concentrata," which is intended to contain from 1.3 to 1.4 parts of anhydrous hydrocyanic acid in 1000, and the dose of which is three grammes. "Amylum arrow," syn. arrow root, arrow stärkelsö (arrow starch), and the synonym for unguentum cetacei, "cold cream," catch the English eye. In conclusion, we must again commend the care and accuracy with which this very useful work has been prepared for publication.

Brognat on Leprosy.¹

THIS pamphlet deserves notice as another proof of the interest which the subject of leprosy has recently excited in the profession. Although it is published in Paris, the author, we presume, resides in Holland, as he wrote about two years ago in the Dutch language a memoir respecting the disease. His data are derived almost entirely from the experience of his father, who is an esteemed medical practitioner in Surinam, where the disease is endemic and extremely common. The information about this colony of Holland (which forms part of the province of South America known as Guiana),

¹ *De la Contagion seule cause de la Propagation de la Lepre.* Par le Dr. C. BROGNAT-LANDRE. Paris, 1869. 8vo. Pp. 80.

constitutes the chief value of the present work. Whether his father shares in the opinion that 'contagion is the only cause of leprosy,' and that it is seldom, if ever, of hereditary origin, does not distinctly appear. He candidly admits that his views are not in accordance with those of several of his countrymen who have within the last few years written on the subject, which, it seems, has on various occasions attracted much attention in Holland from the great prevalence of leprous maladies in her oriental as well as occidental colonial possessions, and from the not unfrequent occurrence of cases in the mother country among the families of European or creole colonists after a lengthened residence abroad. The author saw as many as ten such cases in Holland two years ago. In 1847, and again in 1851, the Royal Institute of the Netherlands drew up a report at the request of the government on the means to be employed for the diminution of the disease in the colonies, and for preventing its transmission to Europe. When the emancipation of the slaves in Dutch Guiana took place in July, 1863, it was found that their total number was 33,560. At that time there were 362 lepers in the lazaret or leprous hospital. This number is exclusive of the cases of disease among the negroes and other inhabitants who were free, so that it is not improbable that the total aggregate of leprous persons in the colony at that time was nearly double that stated above. As reference is made by the author to numerous recent publications in Germany and Holland respecting the disease, his pamphlet may be consulted with advantage. He seems to be only imperfectly acquainted with the late report of the College of Physicians of London.

Martyn on Hooping-Cough.¹

THE patent purpose of this treatise is to prove that the author has obtained the key to the pathology of hooping-cough, the want of which has involved all previous seekers to solve it in vain pretences and attempts. To show that his predecessors have missed their purpose he enumerates thirteen assigned causes of pertussis; many of them, indeed, such as no writer of the present day possessing the shallowest acquaintance with physiology and pathology, would be so absurd as to suggest, and such consequently as should not have appeared in a list of causes supposed to represent current etiological opinion.

It, however, brings out, as intended, the charming simplicity and

¹ *Hooping-Cough; its Pathology and Treatment.* By PATRICK MARTYN, M.D., &c. 1869, pp. 61.

clearness of the author's hypothesis as contrasted with all such previous gropings in obscurity. Hooping-cough is neither this nor that fancied condition, "but inflammation of the upper part of the trachea, aggravated by the peculiar conformation of the adjoining and continuous outlet the larynx" (p. 30), and (to make the pathological picture more complete by an extract from a previous page—p. 26) productive of "a thick transparent glutinous secretion." This secretion, "when it amounts to a certain bulk in that part of the trachea which partakes of the great sensitiveness and nervous associations of the larynx, excites coughing for its expulsion. The glottis is too small—only three lines long up to the age of two years—to let the tough and sticky mass pass. The cough is reiterated again and again with great rapidity and violence to force it through. It cannot; the sense of suffocation is awful; the lungs are exhausted of air; the air rushes in to fill them, and with such force and velocity as to set the chordæ vocales in vibration, and to produce the hoop, and to carry back the partly-expelled secretion; and until the secretion is driven out the cough is continued and the hoop repeated."

At p. 16 he asserts "the cough and the hoop are both the production of the glottis;" but "that there is no spasm the pitch and volume of the tone tells, which proclaims the width of the glottis, one being the measure of the other" (p. 29).

"The only spasm in hooping cough is (he continues) that of the cough, which is remedial;" for spasm "is never a disease in itself, but always useful and remedial." In pertussis it expels the secretion, and has its seat in the muscles of the chest, but for the potent action of which every severe case of the disease would be fatal by suffocation.

The author devotes a chapter to demonstrating the power of the chest as a "mechanical engine," and to illustrate its power of resistance when filled with air recounts a story that might rather have been looked for among the adventures of Baron Munchausen, had he not referred to it as a fact passing under his own observation. It thus stands:—"I have picked up a bombardier knocked down by a 32-lb. cannon-ball, spent, of course, but still having velocity and momentum enough to crush in his chest and kill the man had it not been distended and the glottis closed." But though a knock down and a bruise were the only consequences of this thump on the chest with a 32-lb. ball, the marvel is increased when we read that the doctor had drilled this man, along with his comrades, to keep the eyes open against approaching balls, the glottis closed, and the chest expanded, to receive their knocks, and so enable them to live to fight another day. "He was an Arab (the history continues) of great strength of frame and presence of mind; and I had previously taught him, as I used to do others with whom I served, to keep the chest expanded and the glottis closed when exposed to any

encounter." The Arab certainly was an apt scholar and richly deserved his escape; but he must also have been gifted with sharp eyes to have kept so good a look out for flying balls, and to have got himself up in time to the right degree of chest expansion to receive them, for there is a limit in time to the possible closure of the glottis.

To return, however, to the writer's pathological doctrines. He denies the occurrence of spasm of the glottis because of its small dimensions in infancy. The opening, he argues, is then "so small that reducing it further by spasm would close it altogether, and stop breath and voice for ever." This certainly is an extraordinary argument. What pretence is there for denying the possibility of spasm because of the smallness of an organ? The necessary tissues are at hand in the glottis in the shape of nerves and muscles; and according to his own exposition of spasm as an involuntary contraction of muscles caused by irritation of nerves, the muscles and nerves of the glottis must be exposed to its occurrence, being in all respects endowed like those of other organs. Indeed, they must be more prone to it by reason of their natural excessive sensitiveness.

But Dr. Martyn himself overturns his own hypothesis of the freedom of the glottis from spasm. He believes it to occur "where some minute matter difficult to remove, as a portion of bristle and the like, sticks in the glottis of an adult, and greatly irritates it." Why introduce the qualification to this statement by the words, "of an adult." What will occur in an adult may occur in a child; for according to his own showing the larynx of a child is complete in all its parts, though immature.

It is idle to discuss this question of spasm in the glottis of children further. That unusual temporary and irregular contractility or spasm may occur in the smallest tube possessing contractile tissue and nerves, is an undoubted fact in pathology.

The author is as reckless in other assertions. For instance, he ignores the occurrence of hooping-cough in adult life, though witnessed to by every medical man of experience. "It is (he says) a disease of childhood only," incidental only to the few first years of life (p. 41). He takes no heed of the shrill inspiratory sounds in pertussis, and in discoursing of possible spasm leaves laryngismus stridulus out of sight.

Further, the doctrine for which he contends and which he arrogates as peculiar to himself, that pertussis is essentially an inflammation of the trachea and adjoining parts, productive of a tenacious secretion, is one of old date, and well advocated years ago by Guersent and others. It may also be pleasing to Dr. Martyn to learn that Niemeyer propounds a like opinion, though he certainly is wise enough not to deny the occurrence of spasm.

The recommendations for treatment partake of no novelty, but

repeat those of many past years. Regulate the bowels by calomel with magnesia or rhubarb, and give a mixture of nitrate of potash with tartrate of antimony, to reduce the inflammation and promote the discharge of the secretion, are the crude directions furnished in this special treatise for dealing with this very variable disease.

But the general notice of treatment is marred by some very improper remarks on the remedies proposed by others—remarks of the character we are accustomed to meet with in the writings of those outside the professional pale—calculated to bring discredit upon legitimate medicine. He is scandalised at “at the most deadly poisons” being tried in this disease. “One wonders (he goes on to say) that responsible men should incur the risk of using such drugs with such delicate and susceptible subjects, most of whom have not yet speech to express their feelings, or to exclaim against the wrong done them. Who knows the nature or the effect proper of nux vomica or strychnine on the animal economy, or its curative effects in disease? . . . Let those who incline to it try it on themselves first, or on their children if they love them. The most we know of it is, that it is in great request as the most deadly and immediate poison, and one difficult to detect, by intentional poisoners. Most of the antispasmodics and powerful sedatives I have named are only less hurtful.”

There is much other equally foolish writing, and from title-page to the postscript a pervading spirit of vanity and conceit unbecoming a seeker after truth.

Hartshorne's Essentials of Medicine.¹

A compilation of the character of this treatise is chiefly addressed to the younger members of the profession anxious to learn “the essentials” of physic sufficiently for the purpose of passing examinations. It is necessary, therefore, that the matter be concise and sufficiently on a level with the pathology and therapeutics of the time. We can attest the conciseness and extent of the information given in this work, whilst the known talents and experience of Dr. Hartshorne, and the post he holds as a professor in the University of Pennsylvania, furnish a guarantee of the quality and general accuracy of its contents. In preparing, moreover, this second edition he tells us he has introduced an account of the sphygmograph, and whatever improvements in practice the press has made known during the previous eighteen months.

The work commences with an introduction reviewing the systems

¹ *Essentials of the Principles and Practice of Medicine. A Handbook for Students and Practitioners.* By HENRY HARTSHORNE, M.D., &c. Second Edition. 1869. Pp. 452.

of medicine that have been propounded, and is divided into two parts, —one devoted to the “principles of medicine,” the other to “special pathology and practice.” The first part comprises sections on general pathology, on semeiology, on general therapeutics, and on nosology. In the second part are described in order, the various diseases recognised, their symptoms, course, and treatment.

Here and there in the treatise the author expresses his individual opinion and notes his own experience; and in such a sketch of the whole circle of pathology, therapeutics, and symptomatology, it must be supposed that in some instances the information is not so precise and extended as might be wished, and that some diseases which have less attracted the writer's attention are more superficially treated than their importance deserves.

As examples of Dr. Hartshorne's own opinions we may refer to his remarks on blood-letting, and on what he terms ‘stimulism,’ or the administration of stimulants in disease. The reaction against blood-letting he believes to have gone too far, and he advances arguments against the views of Virchow and Professor J. H. Bennett; and to define his notion of what the remedial action of blood-letting is, calls it “balance.” Altogether his observations on this subject are well deserving study.

In the matter of stimulism, as he understands it to have been advocated by Dr. Todd, it is his opinion that “three distinct propositions are confounded, viz. :—(1) That all disease is debility; (2) That all debility should be treated by the use of stimulants; and (3) That alcohol is always the best stimulant. Granting, with some qualification, the first of these, we emphatically deny the truth of the second and third” (p. 107).

We commend to Dr. J. H. Bennett's notice the recorded opinion of Dr. Hartshorne, that “the mortality of pneumonia has increased in Philadelphia since blood-letting has been so generally abandoned” (p. 134). To this statement he appends a footnote that “the above language expresses my very strong convictions,” and intimates that his notes of private and hospital practice furnish no record of his ever having lost a case of *uncomplicated* pneumonia. His practice is eminently antiphlogistic; bleeding general or local, and tartar emetic constituting the basis of the treatment pursued. The like practice he recommends in acute pleurisy.

The section on bronchitis is, in our estimate, one of those of too general and superficial a character, considering the importance and frequency of the disease. One example of odd grouping is the insertion of the notice of aphasia among diseases of the organs of respiration. The account of jaundice is incomplete, and although dilatation of the gall-bladder is briefly noticed, we find no reference made to ulceration and perforation of that viscus, or of its duct, or of the common duct.

The history of Bright's disease is also not so complete and exact as might be wished. He says, "the consideration of the different varieties of renal degeneration would be too complex a subject for these pages." We think not, and particularly as the recognition of the varieties of chronic Bright's disease is of practical importance both as regards treatment and prognosis. In the account of apoplexy and paralysis again no notice is taken of embolism as a cause, and the author falls into a common-place error in describing the general paralysis of the insane as a condition that comes on at an advanced stage of chronic mania, melancholia, or dementia.

It is an ungracious task to pick out defects in a book that possesses so much merit. We shall, therefore, not adduce others than those already noted, and which may suffice to induce the author to go more carefully into the revision of his teaching in preparing a future edition. In a work that will chiefly be consulted by the younger men in the profession it is especially necessary to bring prominently out those great pathological facts that must regulate practice, and to avoid errors in unfolding those facts; for it is more difficult to unlearn an error than to acquire new facts.

It is, however, due to Professor Hartshorne to recommend his "essentials of practical medicine" as an excellent summary or brief text-book of physic.

Original Communications.

I.—On Spontaneous Combustion. By ALEXANDER OGSTON, M.D.
Aberdeen; Assistant-Professor of Medical Jurisprudence in the
University of Aberdeen, and Ophthalmic Surgeon to the Aber-
deen Royal Infirmary, &c.

FAMILIAR as we are with the minutest details of the structures forming the human body, we have still much to learn regarding their physical and chemical properties. In some important points, our knowledge of these properties amounts to almost nothing, so that many questions have to be left unanswered, and, indeed, will not be capable of being answered, until we shall have learnt more of them.

To take one or two of such still unanswered questions, in which observation has abundant space to assist the judgment, and in which experiment could not fail to solve the problem, we have still before us the unanswered inquiry as to whether the entrance and exit wounds of projectiles from small arms really show the marked differences so long described by every writer on surgery. On this subject observations have been numerous, but have also been contradictory; but, on the other hand, a few experiments, where the observer was in a position to modify the circumstances and eliminate sources of error, could not fail to determine whether the physical properties of the tissues, or the different forms and velocities of the projectiles, were the causes of the differences of opinions prevailing on this point.

Another point about which we are still in the dark is whether, apart from muscular action, dead bones are more difficult to fracture than those in a living subject? It has been stoutly maintained by some (Caspar) that a pistol bullet, capable of penetrating the skull of a living person, will, if projected with the same force and velocity, and under the same circumstances, barely indent the cranium of a dead body. In this, too, we obviously stand in need of increased knowledge of the physical properties of our tissues.

Nor are we on a better footing as regards the chemical properties of our frame. We are still ignorant of the extent to which fire is

capable of acting on the body, and of the possible differences of its action in different states of the system. To our want of knowledge in this respect, we owe it that the subject of spontaneous combustion and of increased combustibility of the human body, has excited so much discussion, and given rise to so many differences of opinion.

In many of the older writers, whose remarks were penned in the infancy of several of the sciences to which we now owe the great clearness and definiteness of our views of diseases and morbid states in general, there occur passages and details of cases leading to the question as to whether or not it is possible for the human body spontaneously to inflame, or, being once inflamed, spontaneously to continue to burn until the tissues are reduced to ashes.

As facts and observations accumulated, the subject began, in the latter half of the eighteenth century, to be worked into some shape by many continental writers, and their investigations were still further rendered prominent by the subject being repeatedly, and in cases of extraordinary interest, brought before the public and the profession in the law courts. Among these writers may be mentioned Lair, Vigne, Marc, Kopp, Lecat, and some twenty or thirty others, to whose researches we owe it, that numerous and minutely detailed reports of occurrences bearing on this question have been collected and rendered accessible. During this period, a general interest, not confined to the medical profession, was brought to bear on the subject, and the various periodicals, scientific and otherwise, teemed with cases, papers, and discussions, on so interesting a subject. This interest and these investigations were continued into the earlier years of the nineteenth century, at which time we find medical jurists and medical men of the greatest eminence engaged in working out the problem, and contributing to a better understanding of it. At this time, the belief in the possibility of such an occurrence was universal, with the exception of an unexpressed scepticism in the minds of the profession in England, with whom the notion of such a possibility has been at no time general or popular. And in the nineteenth century, the same belief in the facts advanced, and concurrence in the conclusions drawn, were the result of the scrutiny which the subject underwent in the hands of such men as Foderè, Orfila, Gordon Smith, Paris, Briand, Breschet, Devergie, Henke, Apjohn, and a host of others. Dupuytren, who also gave his attention to it, was, however, inclined to believe that there existed merely an increased combustibility of the tissues in certain cases, and not, as had been assumed, a capacity for occasional spontaneous ignition.

The new school of legal medicine, originated in Germany by Caspar, refused any belief to either of these views, and his opinions were confirmed by Liebig, who in 1850 published a pamphlet on the subject, setting forth, as had previously been mentioned by

Fontanelle, that portions of flesh soaked in alcohol will burn only so long as the alcohol burns in them, and asserting that the combustion of such a structure as the human body, containing 75 per cent. of water, is an absolute impossibility without the aid of a large amount of neighbouring combustible material. The works of the last-mentioned writers have given to the subject all the features it presents at present, and have stamped upon the minds of those who have, in recent years, adverted to the question, the tendency to incredulity observable in the treatises on legal medicine of Stillé, Guy, and Taylor. One fact, however, is rather remarkable, that none of those who totally disbelieve in the idea of spontaneous combustion, profess to have seen a single case analogous to those observed by its supporters.

There cannot be the least doubt that *the weight of authority is in favour of spontaneous ignition*, or, at least, of increased combustibility, two modes of viewing the subject, which have been generally combined under the title of spontaneous combustion, or *empresmus*. Of all the fifty-four writers on the subject whom I have been able to discover, the opinions of thirty-five of these are contained in the sources to which I have had access. Of these thirty-five writers, five are entirely sceptical, viz. Drs. Caldwell, Caspar, and Taylor, and the chemists Bischoff and Liebig; three believe in increased combustibility, viz. Dupuytren, Stillé, and Guy; while the remaining twenty-seven, including the illustrious names previously mentioned, believe in the spontaneous ignitability of the human body.

Many theories have been framed to account for such a supposed occurrence. The most prevalent has been that held by the earlier writers, and supported by Orfila, Foderé, Gordon Smith, Paris, Briand, &c. &c., that, under certain circumstances, the body is capable of generating under the skin, and in the connective tissue and cavities of the body, hydrogen or other gases similar to those formed in the intestines, and that the electrical condition of the body can sometimes ignite these gases. What these gases are has not been clearly stated; Gordon Smith being of the opinion that carburetted hydrogen was the chief compound, and others, such as Averardi and Apjohn, believing it to be phosphuretted hydrogen.

Another theory, advocated by Lair, Ritter, Kühn, and Mitchell, and based on the fact that most of the victims of this occurrence have been drunkards is, that alcohol was present in their blood to such an extent as to be combustible. Now Liebig and Bischoff say that alcohol cannot be present in the blood and tissues without coagulating their albumen. But this is not true; I myself have seen cases of death from alcoholic poisoning, or in people under the influence of alcohol, where the smell was strong in the blood, and sometimes so marked in the ventricles of the brain, that it was possible to ascertain the nature of the beverage used. Chemistry,

too, has frequently detected alcohol in the blood, and in a case observed by my father, there existed so much alcohol in the body that the serum in the ventricles of the brain caught fire and burned on the approach of a lighted match. Marc and Scherf declare that the eructations and breath of drunkards are occasionally capable of taking fire, though this point seems rather doubtful. But even admitting the presence of large quantities of alcohol in the body does not make this theory tenable, as the experiments of Fontanelle, Liebig, and Bischoff on flesh soaked in alcohol sufficiently demonstrate.

An examination of all the literature of the subject accessible to me shows that there are recorded, in all, between fifty and sixty cases (fifty-seven is the exact number) bearing on the occurrence of *empresmus*, or spontaneous combustion in the human body. Some of these are of old date, from the middle and latter end of the seventeenth century, some are based on very questionable authority; but many are of recent date, and are undeniably correct and conscientious accounts of events which really happened. It seems profitable, in studying the subject, to divide the cases on which the theory of spontaneous combustion has been based into two classes. In the first class may be placed those manifestly false, or where the statements bear absurdity on their face, as well as those the truth of which may justly be suspected until fresh and better evidence shall have been collected to prove or disprove them. In the second class may be placed those whose accuracy cannot be denied; and it will be seen from this division that the deductions based on the authentic cases are very different from those which can be deduced where all the cases, true and doubtful, are admitted indiscriminately as evidence.

The first class, or spurious cases, forms only a small per-centage of the whole, but they are based on the most unreliable data, as a few quotations of them will show.

The first case I shall adduce is detailed in a Florentine journal of the date October, 1776, by Joseph Battaglia, a surgeon at Ponte Bosio, and is cited by most of the writers on spontaneous combustion. I quote Foderé's account, taken from the '*Journal de Médecine de Paris*,' September, 1786.

"Don Gio Maria Bertholi," says the surgeon Battaglia, "a priest residing at Monte Volere, in the district of Fivizzano, went, on the 25th September, 1776, to the market of Filetto, where he transacted some business. After having spent the whole day in travelling about the neighbouring country, he went in the evening to Fenille, and dismounted there at the house of one of his brothers-in-law. On arriving, he asked to be conducted to his room, and, when there, had a handkerchief put between his shoulders and shirt; he was then left alone, and betook himself to his breviary. After the lapse of a very few minutes, an extraordinary noise issued from Bertholi's

apartment, and this noise, in which the cries of the priest were audible, collected the inhabitants of the house, who, on their entrance, found Bertholi stretched on the floor, surrounded by a light flame which disappeared as they approached, and finally vanished. He was immediately carried to bed, and received every assistance that could be rendered him. On the following morning I was called, says Battaglia, and, having carefully examined the patient, I found that the skin of the right arm was almost entirely detached from the flesh and hanging, as was the skin of the forearm."

It is hard to say whether this means simple vesication or more. The account continues—

"On the space between the shoulders and the thighs the integuments were as severely injured as those of the right arm; there was, therefore, nothing for it but to proceed, conformably to the ordinary practice, to remove those shreds; and, perceiving a commencement of mortification in that part of the right hand which had been most injured, I hastened to scarify it, but, in spite of this precaution, I found it next day, as I had feared, in a state of total mortification. On my third visit all the other injured parts were equally sphacelated. The patient complained of burning thirst, and was agitated by horrible convulsions. He passed putrid bilious matters at stool, and was, besides, wearied by continual vomiting, along with much fever and delirium. Finally, on the fourth day, after two hours of comatose stupor, he expired, without showing, in all the course of this cruel illness, any sign or indication of symptomatic pain, or any other affection of that sort. At the last visit I paid him, and during which he was plunged in the lethargic sleep I have mentioned, I was astonished to observe that putrefaction had already made such progress that the patient's body exhaled an unbearable stench. I saw the worms which came from it crawl even out of the bed, and the nails detached themselves spontaneously from the left hand.

"Having carefully questioned the patient as to what had happened, he told me, assuring me of the truth of the facts, that he felt as it were a stroke of a club dealt him on his right arm, and that at the same time he saw a flash of fire seize on his shirt, which was instantly reduced to ashes without the wristbands being touched at all. The handkerchief which, on arriving, had been put on his shoulders between the shirt and the skin, was entirely uninjured and bore no traces of fire. The trousers were equally intact, but the cap was entirely consumed, without a single hair of the head being burnt. The night was calm, the atmosphere surrounding pure, no empyreumatic or bituminous smell was perceived in his room, neither smoke nor the smallest vestige of fire was observed; only the lamp, which had been full of oil, was dry, and the wick in a state of incineration."

In this case the man was not seen by the surgeon till the next morning, so the truth of the facts unobserved by him depends on the veracity of the patient himself, and of the witnesses who saw

merely the flame. It will not have escaped observation that there are many contradictory statements in the account. The priest himself stated that his shirt was instantly reduced to ashes, while his wristbands were untouched; and it is added that there was no smoke or smell, an assertion rather incredible when it is remembered that his shirt and cap, as well as the oil and wick of the lamp had just been consumed. That the man was injured by burning is beyond doubt, and it seems problematical as to whether lightning could explain the case; a more rational supposition is that he had spilt over himself a quantity of spirits, in which, perhaps, he had been in the habit of indulging; that he had caught fire at the lamp, and invented his story to conceal his evil practices. The state of his clothes observed by the witnesses is explicable if burning alcohol had been at work; and it will be observed that his person had been burnt where the garments had presumably been soaked with alcohol, and that his right, not his left, hand was affected. That some of the clothes were left unconsumed will astonish no one aware of the temperature at which alcohol boils, 173° Fahrenheit.

A second case, detailed by Blancard in his 'Collectio Medico-physica,' (Leipzig, 1690,) runs thus:—

"A brandy drinker died in Friesland of spontaneous combustion in July, 1681. He came home at night intoxicated, and lay down in bed insensible with his clothes on. In the night he cried out that he was burning, sprang out of bed, and threw himself on the floor resting against a wall. His wife quickly lighted a candle. He was in a dreadful state, his flesh as if torn from his body with hooks, his whole trunk covered with blisters, his head swelled and burnt black, his hair singed, his nose and ears black, shrunken, and as hard as horn. His clothes were very much burnt, and the tin buttons on them melted. Remarkable to state, at the places where his clothes were completely burnt the body was uninjured, and *vice versâ*. His thighs were burnt so deeply that the unfortunate man did not feel even the deepest incisions. One foot was burnt entirely to powder, and the penis was burnt as hard as horn and shrunk together. Death ensued five days later."—('Encyc. der. ges. Staatsarzneikunde,' p. 734).

This case might be ranked among the credible, instead of among the incredible narratives, were it not for the astounding and self-contradictory statements that he went to bed with his clothes on, that he was burnt on the head, *body everywhere*, thighs, and foot, and yet that he had escaped where covered with clothes; in other words, the parts not specially mentioned as being burnt are the arms and shins, and how a man could go to bed with his clothes on, and yet be clothed only in these parts, is difficult to conceive.

Scherf ('Kopps Jahrb. d. Staatsarzneikund Th., 4 and 6) mentions

a similar case where only the unclothed parts were burnt, but I have been unable to obtain details of it.

A fourth case is narrated in the 'Encyclopädie der gesammten Staatsarzneikunde,' page 733, thus :

"The example of a spontaneous combustion, though only partial, occurred in the village Leognan, eight miles from Bordeaux, on the 5th September, 1822, and was reported in the local French papers by a merchant of Bordeaux of the name of Leon. A smith, named Reynatau, belonging to Leognan, went on the above day on foot to Bordeaux. The day was very warm, the thermometer stood at 30° Reaumur, and the aspect of the sky seemed to presage a storm. In the afternoon he was returning, walking rather fast on an exposed road where the sun's burning rays beat on him. The smith's dress consisted of new cloth; he carried no inflammable substance (*sic*), such as oil of vitriol or aquafortis, on his person; he had eaten only a moderate dinner, being of temperate habits, never abusing spirituous drinks, forty years of age, of strong bodily frame and choleric temperament. When he had arrived at within a mile of his house, at four in the afternoon, it seemed to him, as he was turning his body, as if he received a blow on the right thigh; and he observed on the index finger of the right hand, which hung down on that thigh, a bluish flame, which communicated itself to the middle finger. He immediately put his fingers on his trousers to extinguish the flame, but only communicated the flame to them. Reynatau then threw himself on the ground, buried as quickly as possible his fingers in the sand, and put sand into his trousers' pocket, which the flame had now reached. The fingers of the left hand then caught fire. In this state he went home, and dipped his fingers repeatedly in cold water, but without extinguishing the blue lambent flames, though this was finally accomplished by excluding the air. Fully two months elapsed before the inflammation and suppuration which came on in the fingers were cured. The burning was confined exactly to the point phalanges of the fingers."

This case, on the authority of the smith Reynatau, we are, I think, entitled to disbelieve.

A similar very remarkable case is said to have had its consequences observed in the Hamburg General Hospital, and is narrated in Hecker's 'Literarische Annalen der gesammten Heilkunde,' 2nd volume :

"A young girl, 17 years of age, of delicate frame but healthy aspect, who had menstruated scantily and with difficulty, but quite regularly since her thirteenth year, had suffered for a considerable time from giddiness and headaches, on account of which she had to renounce her occupation of servant, and lived by sewing. She had undergone the usual diseases of childhood easily and without bad consequences. On the 21st January, 1825, as she was busy sewing in the evening, she suddenly felt an unusual rapidly increasing heat

in the whole body; and in the index finger of the left hand, as she was removing wax from the window, a violent sensation of burning. At the same instant the finger was surrounded by a blue flame, an inch to an inch and a half long, which emitted a peculiar sulphurous odour. Neither water nor a wet towel wrapped round the finger extinguished the flame, and, as she repeatedly dipped the finger in water, the whole hand appeared to burn. The girl now hurried home, wrapping, in the mean time, her hand in her apron. The apron and her clothes took fire, but the flame was visible only in the dark. At home she applied milk continually all night long, after which the flame disappeared, but left a painful sensation of burning in the hand, with a frequently emitted sulphurous odour. After venesection and some medicines she improved, but retained still a painful sensation of burning in the left forearm, with occasional sulphurous odour. She was received into the Hamburg General Infirmary on the 25th February"—more than a month later.—"The palm of the hand was at this time studded with blisters; there was a larger one visible on the middle finger; and on the following days a new blister had developed itself on the point of the ring finger, after a renewed sensation of burning. The course of the blisters was rather slower than usual. There were present at the commencement, in addition, some gastric symptoms, and the patient *shrunk frequently together*" (this, I suppose, means rigors or syncope; but it is immaterial). "The thermometer indicated a higher temperature of the left than of the right hand; on the 27th February the left hand was at 25° , the right hand, on the contrary, only 17° (88° and 70° , Fahrenheit). The best electrometers were unaffected when the patient sat on an insulated chair. The disease lasted till towards the end of March, gradually diminishing, and was then finally cured" ('Encyc. d. g. S.,' p. 738).

Even Devergie, a believer in spontaneous combustion, was staggered by and disbelieved this case, which is attested by no medical man. It is certainly fair to doubt whether the young lady, the subject of such unpleasant experiences, had not attached an imaginary history to her blistered fingers.

A few other such cases are on record, but none deserving more attention or belief than those I have quoted; and certainly there seems to be no reason for seeking to found a theory of spontaneous combustion on such worthless data.

Before passing on to the next set of cases, it may be as well to remark that cases of the above class are those on which alone the doctrine of *spontaneous ignitability* rests; those to follow, whose veracity cannot be impeached, speak merely for *increased combustibility*.

The second class of cases, too truthful in their narratives to be disbelieved, and attested by so many competent observers, presents a character differing much from the fables cited above. In the first class, many of the patients recovered; in the second class, to which

I would now direct attention, the subjects all died; and not only so, but were all *found dead*—their bodies, their clothes, and the articles in their neighbourhood, being partially or entirely destroyed by fire, the only remarkable thing about them being that the bodies were burnt and charred out of all proportion to the destruction of the neighbouring objects, and to an extent which seems incapable of being accounted for by the heat of the burning clothes and objects in the vicinity. For illustration, the following specimens of cases of the second class may be cited :

“On the 22nd February, 1821, Dr. Tolson and the surgeon Lelarge, of Beauvais, were ordered by the local magistracy to go to the house of a certain man of the name of Vatin, whose corpse had been found greatly destroyed, and to ascertain the cause of his death. ‘We arrived there,’ says Dr. Tolson, ‘at nine o’clock in the morning, shortly after the occurrence, and received from the neighbours the following account:—Vatin, formerly a beer-brewer, and over sixty years of age, had lived for a considerable time in a very retired and inactive manner, had been in the habit of indulging in spirituous liquors, and had, on the left side of his head, a malignant sore, which often gave rise to hæmorrhages, and was, perhaps, one of the causes of his having already, some time previously, attempted suicide by burning charcoal, and having thereafter spoken to various persons of his intention to repeat the attempt. In other respects his constitution appeared good. He was a tall and very fat man. He spent the evening before his death with a neighbour, and went home at eleven o’clock. A woman who lived in his house certified that he put out his light about midnight, and went to bed. In the morning, towards eight o’clock, a thick smoke forced itself out of the openings of his chamber. The neighbours became suspicious, forced the locked door, and saw his corpse lying on the floor, burning with a flame which it took a good deal of water to extinguish. On our arrival the room was full of thick smoke, which, as well as the corpse, had a disagreeable empyreumatic odour. We found the body lying on the floor, several steps removed from the bed. A chair, the straw and part of the woodwork of which were burnt, lay upturned in the same direction as the body, near a vessel for coal, in which was a small quantity of half-burnt fuel. The water which had been poured out in the apartment contained a good deal of fat. The head of the corpse was still attached to the neck, the flesh of which, behind and laterally, was destroyed to the bone. The cervical vertebræ were unaltered. The face was puffed up and blackish-red, as in death by suffocation. On the left side the upper extremity and the wall of the chest were destroyed, and only calcined portions of the ribs and arm remained. The back parts of the ribs and the right shoulder and arm were present; but the hand, which from the bend of the forearm lay on the region of the stomach, was destroyed along with a part of the forearm. The spine was preserved as far as the lateral processes of the left side of the vertebræ. Of the contents of the

chest and belly we found only the lungs, heart, and liver, dried and shrunk, but still preserving their usual form; their substance was bloodless. No trace remained of the other entrails. The lumbar vertebræ were much injured, but were still attached to the pelvis, of which, however, only the right os innominatum preserved its texture. The left thigh was completely destroyed, the leg was separated at the knee-joint, and showed there only marks of an ordinary combustion. The right thigh was burnt, but the bones preserved their continuity, in spite of the destruction of the muscles. We found nothing else in the room that had caught fire, except the coals, which the deceased had purchased on the previous evening. This extensive destruction of a body in so short a time, and with substances so little combustible, hardly admits of any other explanation than that Vatin was first suffocated, and his body, which must have been unusually combustible, had come in falling into contact with the embers, and so was destroyed'' (ibid., p. 735).

Second case (from B. Wilmer's cases and remarks in surgery, London):

"A woman was excessively addicted to brandy drinking. Her bed stood about three feet from the fireplace. One morning she was found burnt to ashes. Between the bed and the fireplace lay her remains—the feet, a thigh, and some bones. The furniture of the room was very little injured by fire. It was supposed that she had fallen out of bed at night, and that her chemise and unusually combustible body had been ignited by a candle which stood in the fireplace" (ibid., p. 736).

Dr. Proteau, in the '*Salzburger Med. Chirurg. Zeitung*,' 1815, Bd. I, p. 284, details the following case:

"An extraordinarily fat woman, twenty-nine years of age, and much given to spirituous drinks, so as often to consume a bottle and a half of brandy a day, fell a victim to spontaneous combustion; and Dr. Proteau concludes, from the circumstance of the case, that the combustion took place from within outwards, that the clothes were not ignited till near the end, and that there was no external cause for the occurrence" (ibid., p. 736).

Dr. Proteau's conclusions are, however, rather rash, but do not invalidate the case.

"On the 12th of January, 1820, at 10 o'clock in the evening, the neighbours of a Madame P—, in Nevers, remarked a peculiar disagreeable odour, as of burnt animal substances and burnt wool; but as no smoke or steam was observed issuing from any of the adjoining houses, they retired quietly to rest, with the idea that the smell originated in the remains of some burnt rags from a Carmelite nun who had died that day in the vicinity. On the morning of the 13th, a neighbour who had a key of the house, because she was in the habit of coming in the mornings to help the servant maid, opened

the house door to do her usual duties. As she entered the room, she was surrounded by a thick smoke with such an unbearable smell that she thought she would be suffocated. She ran out immediately, crying pitifully for help, whereupon the neighbours gathered round the house, and, after they had got the thick smoke dissipated, searched the whole apartment. They saw neither Madame P— nor her servant, and at first no traces of even their corpses, they found only the bed burnt. The different parts of the bed, however, had preserved their form, but were changed into ashes, and fell in pieces at the first touch; bedstead, mattress, feather bed, sheets, coverlet, and curtains (which last two were of wool), and the top of the bed.”

It may be remarked in passing that this account of the neighbours seems somewhat exaggerated.

“Before removing the ashes, the fireplace was searched, but no trace of burning wood or coals found in it; the fire had not been covered up, and had probably gone out from want of wood. One candlestick stood in the chimney, another on the floor in the middle of the room; there was no candle in either, though it was supposed that there had been one in the latter, and that it had been burnt along with the bed. On afterwards searching among the ashes, at the front of the place where the bed had stood, they found the lower part of a leg with a stocking on it and a shoe on the foot; it was recognised to be the right leg of the servant, and was the only part of her not reduced to ashes. The brain-pan of her mistress, half burnt, with neither hair nor skin, was found at the place where her head usually lay. Besides this, a small part of the neck remained; the neck was wrapped round with a red cloth, and portions of this cloth still adhered to the fragment of the skin. The servant’s bed stood near that of her mistress, but was uninjured, as were the chairs, table, and remaining furniture, with the single exception of a wooden wall clock fixed to the wall near the bed, and which had preserved its shape, but fell into ashes on being touched. The room was not plastered, but yet the boards and beams close to the roof of the bed were not burnt, but black and giving out a burning heat. Everything in the room, especially near the bed, was very moist, probably from the condensation of the thick vapour which had filled the chamber. Nobody lived in the house but those two women, so the cause of the occurrence is unknown. During the night the weather had been calm, the air dry, and the frost so sharp that the thermometer stood ten degrees below freezing point (*i. e.* 14° Fahr.). The woman was ninety years old, the servant seventy; they were both weak, thin and wiry, and lived poorly, though the mistress had 6000 francs of income. The woman drank eau de Cologne to great excess, and the servant indulged in wine” (*ibid.*, p. 736).

The details of this case, it will be seen, are not confirmed by medical testimony, and are, no doubt, highly polished.

Dr. Hellis, of Rouen, gives, in the 'Journal Générale de Médecine' for April, 1826, the following case:—

“ On the 31st December, 1820, Dr. Hellis was required to examine into the cause of death of a woman who had been that morning found dead in her room. For that purpose he betook himself, along with the Commissary of Police, to No. 85, Rue des Arpens, where the woman had lived in an attic on the fourth floor, which entered from the court-yard. As soon as the door was opened Dr. Hellis was met by a strong empyreumatic odour, a thick smoke filled the room, and on the floor lay the corpse of a woman in the following condition:— She was lying on her belly, her face towards the floor, and only the legs, thighs, and a part of the buttocks and head recognisable. The breast, belly, and back, had vanished, their place being marked by only a few calcined vertebræ. The left hip lay on a block, which served to support a pulley. This block, as well as the hip, were still burning, although a good deal of water had been poured upon them before the arrival of Dr. Hellis. When Dr. Hellis had pushed away the hip from this block he observed the face, which was uninjured, and covered with a yellowish, fatty, and stinking deposit; the hairs which were removed from the head were entire, as was the cloth which had held them together and secured the head. The back part of the head and neck were converted into charcoal. The upper parts of the shoulder-blades and the space between them were covered with flesh, but the lower half of these parts was calcined and crumbled on the slightest touch. On the front could be seen the collar-bones, some remains of the first and second ribs, and a few pieces of charred matter where the lungs had been. The skin, muscles, and bones, which formed the rest of the trunk, were completely destroyed. There were no traces of the stomach, liver, or bowels, visible. The pelvis was partly destroyed, and contained only a calcined, shapeless mass. The loins, legs, and feet, were uninjured; the clothes were destroyed, only the coverings of the head, the stockings, and shoes remaining. The spectacles of the deceased lay on the above-mentioned block, and their case at a little distance off. None of the furniture of the room was injured. A screen before the fire-place, a cupboard, a bundle of twigs, and a chair at a little distance from the body, showed no trace of the action of fire. There was no fire in the fireplace. Near the hearth stood three vessels for coal, containing no burnt fuel. A candle on the table was found extinguished, but almost entire. Dr. Hellis learnt from the narratives of the neighbours that the deceased Thomasine Goret was fifty-seven years old, and had long been given to brandy-drinking. Her husband had separated from her on this account, but for twelve years had paid her four francs weekly, which she used forthwith to spend in a drinking-shop, and used not to leave the shop till she had lost her senses. On the 30th December she had got her usual donation, but went out to drink on credit, as she wished to reserve the money to do honour to the new year. She was heard to say that the cold had troubled her so much the previous night, that

she was resolved not to go to bed that night, and, to protect herself still better from the inclement weather, she went home at ten o'clock excessively drunk. Nobody was astonished when, towards midnight, she was heard to move and throw herself about. Shortly after the neighbours heard a crackling, as if butter were being roasted. As the sound became louder, and continued some time, they conceived that fire had broken out in the house. Somebody rose, but immediately lay down again, as no particular light was visible. At seven in the morning a neighbour, wishing to borrow something from Goret, opened her door, but was met by a smoke so dense that she could distinguish nothing. She called for assistance, and water was immediately brought, and sprinkled through the room. As soon as objects became visible, to the astonishment of all none of the furniture of the room was found burning, but the unfortunate Goret was lying blazing on the floor. Dr. Hellis says he could not venture to decide whether this combustion arose spontaneously or from contact with a burning body. The woman was very fat, and of sedentary habits" (ibid., p. 737).

To these few cases selected from many I would add a case which occurred in the present year in my own experience. On the 14th March, 1869, my father and I were requested to examine the remains of Mrs. Warrack or Ross, aged 66, who resided alone in a house near the Bridge of Dee, Aberdeen. She was said to have been stout, of intemperate habits, and her son stated that he had left her, at 10 a.m. on the 14th, in her usual health. She was found at 11 a.m. on the same day lying burnt on the lower steps of the stair of her house on her left side. The house was pervaded with a disagreeable smell, but liker that of burning straw than of burning animal matter. The room which she usually inhabited, the door of which was within two yards of the place where she lay, had the same smell; the chair in which she sat stood in the middle of the room, its back almost entirely consumed, and its arms wholly so. The seat of the chair showed mere traces of the action of fire. The bed, about two feet from her chair, had its straw mattress slightly burnt at its fore part. The woodwork of the bed and the curtains were uninjured. Her chair was about four feet from the fireplace, and about two feet from an uninjured mahogany table, on which stood an empty beer bottle smelling of whiskey. Nothing else in the room was touched by fire. The stairs were of wood, and underneath, and in the immediate vicinity of where she lay, they were charred to the depth of a quarter of an inch. The perpendicular bars of the hand rails similarly charred beside her for a foot up, the top rail and the wall, which was half a foot from the hand rail, blackened by smoke. The condition of the body, however, showed that the fire had caused the greatest alterations in it. The hair was burnt off, the soft parts of the face and front of the head burnt off, the bones exposed, blackened, and calcined. The back of the head,

the neck, and the trunk everywhere, converted into greasy charcoal to the depth of about an inch, the skin totally removed, and the bones of the trunk lying bare, blackened, and calcined. The front wall of the abdomen totally destroyed and wanting; the intestines burned into a hard and blackened mass; the liver converted into ashes to the depth of an inch, but retaining its shape, its left lobe projecting nine inches from the margins of the ribs. The upper limbs distorted; the elbows strongly flexed, and everywhere charred to a great depth, the bones, however, even of the fingers, preserving their position. The right thigh had its deeper muscles still uncharred, but of the appearance of roasted beef, and very dry; the skin and superficial muscles totally burnt away. The right leg only partially attached to the thigh, and entirely converted into a greasy, black charred mass, even the bones not escaping. At the lower part of this right leg the bones had the soft parts entirely burnt away from them, and were black and calcined. The right foot totally detached from the leg, and converted into a soft, black, greasy, and shapeless cinder, through which the finger could be pushed with ease. The left thigh and leg in a condition similar to that of the right extremity, but still attached to the foot, which was a charred and shrivelled mass similar to the right foot. Not a vestige of clothing remained anywhere.

This second class may profitably be concluded by the following case by Devergie, which is, however, one of perfectly different import from those which precede it. In it a dying man is supposed to have been burnt by the heat of a dunghill.

“ Charles Francois Francoey, aged 40, commissionaire, Rue de Faubourg Saint Antoine, was brought to the Morgue on Saturday, 20th August, 1831. He was a drunkard, and had, on the previous evening, for a bet, drunk a certain quantity of brandy. Becoming deeply intoxicated, he was thrown on a heap of dung, where he remained long without a sign of life. The face and eyes had the appearance of a man in drink; lips and eyes half open, limbs flaccid. No trace of violence on the exterior, and no sign of putrefaction. The legs and inner surfaces of the legs of a violet hue. Skin of the legs mottled violet and reddish-brown. The epidermis removed over the whole posterior surface of the lower limbs, and over the greater part of the buttocks and back. At all those parts the skin had acquired the inflamed and injected reddish-brown colour of a burn of the second degree. Course of saphenous veins marked on the insides of the legs. By atmospheric contact the denuded parts were dry and horny, like parchment. At a few parts of the legs and thighs, epidermis was still present, and could there be stripped off with the utmost ease, leaving the subjacent dermis and rete mucosum very red and injected. The garments, consisting of shirt, vest, and linen trousers, were unaltered, and the whole anterior surface of the body was intact. At the autopsy, three days after death, putrefaction

had invaded the face, neck, and shoulders, which were green, the veins there being filled with blood, and their tracks marked by green branching lines. Pectoral muscles and neck distended with gas; thighs also emphysematous, the gas existing chiefly in the muscles, deep connective tissue, and round the vessels. Lower part of the abdomen greenish."

The further details of the autopsy show excessive decomposition; and the gas, developed everywhere in the tissues, burnt when punctures were made ('Méd. Lég.,' vol. 2, p. 296).

This case I have cited for the sake of completeness, but believe it to be nothing more than early decomposition, hastened at the back parts of the body by the warm dung. If space permitted I might adduce analogous cases from simple decomposition.

On reviewing the two sets of cases, and the inferences fairly deducible from each, it becomes evident, as before mentioned, that while the *first class*, or untrue cases, *has given rise to the theory of spontaneous ignitability*, the *second class* of narratives countenances no such doctrine, and *speaks merely for increased combustibility on accidental ignition*. The possibility of, or mode of origin of, spontaneous ignition, need not be discussed here, since there is not, in any one of the trustworthy cases, the slightest call upon us to assume its existence, and it would be fruitless to discuss a phenomenon of the occurrence of which we have no evidence.

But that increased combustibility exists cannot be denied, though at first sight it is not so clear to what it owes its existence. The question has given rise, as has been already seen, to numerous hypotheses, all of which, with one exception, are manifestly untenable, and it is owing to the wildness and illogicality of these hypotheses and deductions surrounding the subject, that the whole question has come to be treated as a half-forgotten fable.

Most writers on spontaneous combustion have remarked that those who have fallen victims to it have been usually old women, and generally, though not invariably, fat people; and Dupuytren was led, in the '*Lancette Française*' for February 1830, No. 97, to say in regard to this subject, that he had, in frequently burning portions of bodies, found no difficulty in consuming them, especially the fat parts. "I do not know," he adds, "a single example of spontaneous combustion in a lean and dry individual: all were, without exception, extremely fat." After speaking of the way the victims among his countrymen become stupefied by drink and the carbonic fumes from their warming pans, he continues, "When the fire gains the clothes it burns the skin, which cracks and allows the fat to run out; part of this flows down on the floor, the rest serves to support the combustion, and with free access of air everything is burnt. This is how alcohol is the occasional cause of the combustion; it is in stupefying and rendering the patient insensible that it

acts, and not by the pretended amalgamation with our tissues.” Devergie and others ridicule this explanation, which the clear intelligence of Dupuytren had suggested, and remark that while the flame of fat is white, the flame in cases of spontaneous combustion is blue (a property which belongs, we have seen, to the fabulous cases), while Beck, in a foot note to his ‘Medical Jurisprudence,’ disposes of his views with the words, “Without derogating from his acknowledged talents, I will only add that Dupuytren was a better surgeon and anatomist than a chemist.”

Dupuytren is not absolutely accurate in all he says in the above quotations. He gives alcohol the position of a mere stupefying agent, whereas it deserves to be borne in mind that the prolonged indulgence in spirituous drinks is capable of producing an excessive deposition of fat in the body (Virchow, Huss). But in the doctrine that bodies owe their increased combustibility to excess of fat, Dupuytren has advanced the only explanation capable of setting the subject at rest, and on a true basis explaining rationally and philosophically the cases of so-called spontaneous combustion. That a body burning in a sheet of oil flame is capable in a high degree of incineration in a brief period of time, is proved by the accident at Abergele in August, 1868, where, some railway cars being surrounded by ignited petroleum, thirty-one persons perished, and their bodies were found destroyed in various degrees, the destruction being on the whole similar in extent and degree to that observed in the recorded cases of the so-called spontaneous combustion. The flame by which these bodies were burnt was of only a few hours’ duration.

When we consider the amount of fat some bodies contain, the subject grows even clearer, and a review of the cases demonstrates that the incineration was always most extensive in the skin and subcutaneous adipose tissue and other places where fat is abundant, and least marked in organs and regions with less fat. The fatty degeneration of various organs and structures, the intermuscular and subcutaneous adipose tissue, along with the masses deposited in other parts of the body, so great as sometimes to constitute a layer under the skin two to three inches thick, and in the omentum another layer one to two inches thick, all present a body of oleaginous matter sufficient amply to account for the combustion, and which, once ignited, would tend rather to burn *in situ* than to flow out, thus explaining the greater destruction of the corpse than of objects in the vicinity.

As might be expected from the varying amount of fat in different individuals, the result, in the recorded examples of spontaneous combustion, varies according to the amount of combustible matter—clothing and fat—present; but, by considering each case by itself,

and excluding the tendency to assume some marvellous agency to have been at work, if we do not explain everything to our entire satisfaction, we arrive, at least, at a juster estimate of the nature of the process than has, except by Dupuytren, hitherto been attempted.

It is hardly necessary, in conclusion, to advert to the possible differences in the origin of the fire, and the influence these would have on the result. In some of the cases the clothing may have been of a more combustible character, and more abundant than in others, and may have been chiefly of vegetable character, such as cotton or linen; while in some instances there lies very close at hand the supposition that the victims may have, in their intoxicated state, emptied over themselves their alcoholic drinks, or may even have contrived to cover themselves with some oily and more fiercely burning substance.

NOTE.—In the discussion following the reading of this paper at the Aberdeen Medico-Chirurgical Society, in July, 1869, the following facts were mentioned by Dr. Robert Beveridge, lately Pathologist and now Physician to the Aberdeen Royal Infirmary, whose permission to reproduce them I here gratefully acknowledge.

In a number of experiments as to the combustibility of human tissues, Dr. Beveridge has had occasion to observe:

1st. That, while simple heat or exposure to a red flameless heat occasions but a slow charring and destruction of any tissue, the exposure, on the contrary, to the action of flame, gives rise to a much more rapid process of destruction.

2nd. That, in portions of flesh removed from the human body, if a surface containing no fat, such as the section of a muscle, be exposed to the heat, the charring which results is a slow and very gradual process, while if the cutaneous surface be placed next the flame, the cutis is speedily destroyed and charred, and cracking permits the liquefaction and flowing out of the subcutaneous fat, which, taking fire, envelopes the whole mass in a flame so strong as speedily to reduce it to the condition of a black greasy substance resembling cinder.

3rd. That soaking in alcohol makes no difference as to the result, neither accelerating nor retarding it. When a portion of tissue saturated with alcohol was exposed to heat, it showed at first the bluish flame of alcohol, which speedily passed away without affecting the tissue, the combustion of the latter then proceeding as if no alcohol had been present.

Dr. Beveridge had independently come to the same conclusions as Dupuytren as to the mode of action in the cases of so-called spontaneous combustion, and is of the opinion that in the reported narratives of this phenomenon, the body possessed in no case any

preternatural combustibility, beyond what the amount of fat existing in it gave rise to; so that, while admitting the result to differ in different cases, owing to the above-mentioned cause, there is no reason to believe that any other preternaturally combustible state exists; and that in the occurrence of such cases in habitual drunkards so often referred to, the presence of intoxication acted only in increasing materially the liability to accident, and in depriving the victim of any power of assisting himself or of giving an alarm.

II.—On the Variety of Periosteal Disease of the Skull, generally known as Fungus of the Dura Mater. By LAWSON TAIT, Assistant-Surgeon to the Clayton Hospital.

GEORGE OBERHOUSE, æt. 58, a political refugee from Strasbourg, was under the care of my friend, Dr. Holdsworth, for tubercular disease of the lungs. His case presented some interesting peculiarities, which, however, do not affect the present inquiry. My attention was drawn, on the 7th of May, to a tumour in his right temporal fossa. He had noticed it for eleven weeks, and its appearance was coincident with intense pain on that side of the head, especially when the tumour was pressed upon. It had slowly increased in size, and when I first saw it was about an inch and a half in diameter, slightly irregular in shape, lobulated, soft and presented a pseudo-fluctuation. I gave, as my opinion, that it was a case of fungus of the dura mater, and had better not be interfered with. I thought that I could feel the margin of the aperture in the skull. The tumour slowly increased; and during its progress I carefully watched the case. He died from the phthisis on June 25th, and I examined the head twenty-four hours after death. I did not examine the rest of the body; and this, for reasons I shall afterwards allude to, I very much regret. There were urgent reasons, however, at the time for this neglect.

Careful dissection elicited the following facts:—the tumour occupied the whole of the right temporal fossa, and lay quite loose under the temporal muscle, to which it was not attached. It extended upwards above the junction of the temporal fascia with the pericranium for about half an inch at the anterior part; and at this point it was quite evident that the tumour was intimately connected with the pericranium. The tumour was adherent to the skull, which it had not perforated but much eroded. When the tumour was separated from the bone, a somewhat abundant juice exuded, not from any distinct sacculi, but from the general substance of the tumour. This juice had a reddish yellow or light grey colour, was of the consistence of cream, and was composed of irregularly shaped and sized cells,

containing one or more nuclei. The skeleton of the tumour was composed of interlaced fibres, sparsely distributed. When fresh, the cells displayed a most peculiar power of elongating themselves and altering their shapes, when carried through a strait by a current, such as I had never before seen to an equal degree, save among Infusoria.

This tumour was two inches and three quarters in vertical, and two inches and one quarter in transverse measurement. It was capsulated by the fibrous layer of the pericranium. At the spot corresponding to the centre of this outside tumour the dura mater presented a soft, semi-fluctuating swelling, of the size of half a crown; and on carefully peeling the membrane off the bone, appearances very similar to those already described presented themselves. The same reddish-grey juice, with the same cells and the same basement structure grew between the bone and the dura mater. The middle meningeal artery ran through the centre of the adventitious structure, and round the artery was a tube of the tumour-substance of denser consistence than the rest. The bone presented the same appearance as was found on the outside—not as if carious, but with an innumerable number of holes so small as to be capable of admitting the point of a needle only: in fact, just as if there had been, what I have no doubt there was, a destruction of cell-territories without their renewal. The serous surface of the dura mater presented no abnormality.

My apology for this paper is that the disease, of which the case related is an instance, is so rare that any notice of it, however imperfect, is worthy of record, and any attempt to refer it to its proper position in the nosological table worthy of attention.

Notwithstanding the number of monographs of which fungus of the dura mater is the subject, it is an affliction about the nature of which there still seems to be much difference of opinion; many pathologists, those principally, however, who have not seen much of it, leaning to the view advocated by Louis, that it is a special disease, worthy of a special name; others holding that it is merely a variety of that disease to which, for want of a better, we give the conveniently vague name of malignant disease or cancer. Before the case of Oberhouse came under my observation, I had read ‘Louis’s Memoirs,’ and was strongly impressed with the belief that the former view was the correct one, and I diagnosed the case correctly to some extent, I am glad to say, in that belief. The examination of the tumour, and, still more, two or three weeks’ reading which I devoted to this most interesting subject, have led me to alter my views, and the reasons for the change I purpose to record here briefly.

Lebert and Virchow argue that these tumours are not malignant, and the distinguished pathologist of Berlin bases his opinion principally on the ground that the tumours are solitary, and do not pre-

sent any central softening. That the tumours are not always solitary I shall afterwards show, and I must say that the second reason does not seem to me to have much weight, as many tumours, undoubtedly malignant, do not present any central softening, while some of the recorded cases of fungus tumour would seem to have presented this characteristic very decidedly.

It would seem that the case recorded in 'Louis's Memoir,' Obs. XIX, recovered after a most terrible operation; but we have no record of the after history of the case, so that the question of the return of the disease in this solitary instance of recovery after its complete removal, either in its original site, or as secondary disease elsewhere, is left unanswered.

The period of duration of the tumours is altogether in favour of their classification as cancers; they rarely last more than six months. Louis's own case, Obs. I, would seem, from the dates given, to have lasted over twenty-two years; but there is evidently a clerical error in giving "1763" for "1743," and as he states that the tumour grew rapidly, and that for the last few *months* of his life the patient was under the hands of quacks, it is quite clear that the course of the tumour's growth did not extend over more than sixteen months. A case quoted from M. Robin is stated to have lasted nine years, and Obs. XVI, four years; but with these exceptions, in all the records when the fact is noticed, the progress of the tumour was rapid. The rapidity with which the tumour fungates and grows to more than its original size, after operative interference, is marked in all the cases when this unfortunate treatment has been adopted.

A very usual story, one recorded by Ambrose Parè as a warning to young surgeons, is that the tumour was mistaken for an abscess, and opened, and is generally followed by a disastrous result. The alarming hæmorrhage which always follows this interference is another fact which leads us to the conclusion that the tumours are cancerous. One of the chief characters of these tumours, in all stages of their growth, is the intense pain, generally of a lancinating character; and this again favours the view that they are not benign. The variety of age shown in the history of the cases is suggestive of malignancy: Chopart gives a case of an infant, two years of age; Louis, Obs. XIX, a girl, aged nine; Dupuytren, a boy, aged twelve; one is reported by Cruveilhier, in the livraison huitième of his 'Anatomie Pathologique,' occurring in a man, aged sixty-five; Dr. J. W. Ogle records an instance of the tumour in a man at the advanced age of seventy.

The weightiest arguments for the classification of fungus tumours of the dura mater among the malignant tumours, are to be found in the descriptions of post-mortem examinations, when the cases are recorded with sufficient accuracy and detail to make them worthy of reference. And here I must express my regret that out of the large

number of records of these cases which I have examined, but few are of any value from the absence of detail. In the Obs. VI of Louis's Memoir, a case reported by M. Sivert, the left parietal was occupied by a large tumour, and there were two others smaller, one under the coronal suture, and one under the occipital bone.

In the Obs. VIII, a case under the surveillance of Mr. Ray, a soldier had a tumour accompanied with pain in the hip, and also a tumour perforating the frontal bone. M. Le. Maire operated, and the patient died. "The femur was carnified from its middle part up to the head, which was retained in the cotyloid cavity by the dried round ligament; this altered part was nothing but a fleshy mass."

In the Obs. X, a part of the tumour was found to project inwards, and lodged in the left hemisphere of the brain.

Obs. XV records a tumour, in a male aged fifty, perforating the occipital bone, and another at the base of the skull. The medulla was surrounded by a similar fungus which appeared to fill the occipital foramen.

In the 'Medico-Chirurgical Review' of 1834 there is a notice of a case of a male, aged fifty-nine, in whom the tumour perforated the parietal bone, and was operated on by M. Berard with a fatal result. The post-mortem examination showed that the "exposed dura mater seemed healthy except at the hinder part where it presented several tuberculous granulations of a cancerous appearance; and it was found that the inner table of the skull had been worn away there."

Cruveilhier records a case in a man, aged sixty-five, in whom the tumour perforated the parietal bone: the longitudinal sinus contained vegetations of "medullary matter," and two rounded cancerous bodies grew from the dura mater at the angle between it and the falx; these had deeply indented the hemispheres, the contiguous cerebral matter having disappeared.

In the 'Medico-Chirurgical Review' for 1830 there is a record of a case in which "within the foramen magnum, and attached to the inner surface of the dura mater at its anterior and lateral parts, was a spongy tumour of a greyish yellow colour which, when cut into, presented a variegated structure resembling fungus hæmatodes."

In the same journal, for 1829, is given a case in which the "post-mortem examination discovered a large tumour arising from the dura mater covering the middle division of the right side of the base of the skull, consisting of a number of encysted tubercles filling up the hollow of the squamous portion of the temporal bone, thence (the bony matter which would have otherwise stopped its progress being destroyed) descending into the throat and forming there a large conical pendulous projection. The front of the tumour blocked up the posterior entrance of the nostril, and its right side that of the corresponding Eustachian tube."

In the transactions of the Pathological Society I find that Dr. J. W. Ogle showed a fibro-cellular tumour in January, 1857. The specimen was of the size of a walnut, attached to the dura mater, very vascular and firm, evidently situated within the luminæ of the membrane. The specimen had been removed from the vertex of the head, a little to the right of the superior longitudinal sinus, which it rather interfered with, and it had greatly indented the surface of the brain. The subject was seventy years of age, and had given no signs of its presence during life. The tumour consisted of fibrous tissue, vessels, and vast numbers of large, many-nucleated cells, numbers of which were nested as it were and enclosed in other cells, flattened and epithelial in appearance.

In the 'Lancet' for 1855 Mr. Filliter relates the case of a woman in whom two tumours were found attached to the dura mater, occupying cavities, one in each hemisphere, from which they could be readily withdrawn. In the right posterior pillar of the fornix was a hard nodule. Both hippocampi, but especially the right, were indurated. The tumours of the dura mater were found to consist of fibrous tissue and of fibro-plastic cells, which were not materially altered by acetic acid: there were also free nuclei. The smaller of the two contained numerous calcareous spiculæ.

The late Dr. McKenzie, of Glasgow, has left us a most interesting paper in the 'London Medical Gazette' of 1838, in which he relates the case of a female, aged fifty-eight. On post-mortem examination a tumour of the dura mater was found about the size of a walnut, growing from the cerebral surface of the membrane, and imbedded in a depression in the hemisphere. On raising the brain from the base of the cranium a very extensive mass of disease was exhibited. A large, irregular fungoid mass was found in connection with the dura mater, occupying chiefly the middle and right side of the cranial base. It was of a dirty yellowish colour, and of varied consistence. The cerebral surface in immediate contact with the fungus was completely disorganised to a considerable extent. At the lower and posterior part of the right middle fossa, in the great wing of the sphenoid, absorption of the bone had taken place and produced a roundish opening, with rough, brittle, and irregular edges. Through this aperture the fungoid growth passed out of the cranium, and made its appearance externally at the side of the neck, and this had, during life, been mistaken for a glandular affection posteriorly, and for the most part the disease involved both surfaces of the dura mater.

Dr. McKenzie also gives the case of a male, aged sixty-nine, in whom the tumour seemed to be ten years in growing, and after death was found to have removed nearly the whole anterior floor of the skull without affecting the brain substance. Dr. McKenzie states that fungus of the dura mater attacks either surface of the membrane.

It appears to affect the proper fibrous substance and not the tunica arachnoidea. I believe that the distinguished oculist is in error on these points, as I shall afterwards endeavour to show.

Chelius¹ gives a case in which a pulsating tumour, perforating the frontal bone, was secondary to medullary disease of the head of the humerus; the ethmoid, ungual and orbital plates of the frontal bone were destroyed by the fungus, and the tumour of the humerus grew from the inner surface of the periosteum,—had removed the bone, was capsulated by the membrane, and was identical in structure with the cranial tumour. He also gives the case of another male, where the body of the left frontal bone and its orbital plate, together with the cribriform plate of the ethmoid were affected, the tumour growing, as in my own case, on one side of the bone from the dura mater, and on the other from the pericranium. Here and there the bone was sarcomatous (an instance of his fifth variety of the disease).

Wenzel² found in one case five of these growths penetrating the calvarium and the liver occupied by masses of similar disease. The most convincing instances, and, perhaps, the most valuable contributions to the records of this disease, are two cases published by the late Sir William Lawrence in the '*Lancet*' of August, 1853. The first was a female, in whom the tumour perforated the left frontal. In ignorance it was twice interfered with, and fungated so rapidly that on the second occasion this took place before the wound had healed. Before the third operation, undertaken by Lawrence on urgent request, a deeply-seated tumour was observed in the left hip. This had existed for two years, was moveable, and free from pain. On removal the cranial tumour was found to contain bony spiculæ, and it was covered by a capsule "which might have been partly altered pericranium." She lived a few weeks after the third operation, during which time the tumour in the hip had increased a good deal in size. On post-mortem examination the cranial tumour was found to be about two inches in diameter, and the dura mater was perforated by a hole about half an inch in diameter, through which some of the fungus protruded inwards. A small portion of similar disease, apparently developed in the left pterygo-maxillary fossa, had forced its way through the sphenoid bone into the base of the skull, just in front of the petrous bone. The left lobe of the cerebellum contained an almost spherical mass of medullary disease. A fracture of the humerus had been caused by disease of the bone, and had gone on without any attempt at repair. The upper fragment had formed an elliptical swelling of the size of a hen's egg, the bone being converted into a mass of medullary disease, covered by a

¹ '*Zur Lehre von den Schwammigen Auswüchsen der harten Hirnhaut und der Schadelknochen*,' Heidelberg, 1831.

² '*Ueber die Schwammigen Auswüchse auf der aussere Hirnhaut*,' Mainz., 1811.

fibrous investment from the periosteum, in which there were slender portions of bone. The second case was that of a woman aged 33, in whom the disease had perforated the right parietal bone. It fluctuated and pulsated. Post-mortem examination showed that "the dura mater had undergone extensive fungoid degeneration, similar to that observed outside the skull, occupying almost all the right side, and implicating the parts at the base of the skull, particularly those included in the *cella turcica*. It is separated from the bone in some places to the extent of a quarter of an inch or more by the fungoid growth." In the liver, right kidney, and in both lungs were nodules of what certainly was secondary encephaloid disease.

In my own case there was no symptom to excite suspicion that there was secondary malignant disease in any of the organs. The affection of Oberhouse's lungs was undoubtedly tubercular, but I much regret that I was obliged to be without absolute demonstration of this and of the condition of the other viscera.

In the fourth volume of his 'System of Surgery,' Mr. Holmes speaks of a preparation in the museum of St. George's Hospital showing a fibrous tumour of the skull and dura mater, which had perforated the latter membrane and was lying on the brain, from which it received a very decided pulsation. The patient suffered from occasional fits, loss of memory, vertigo, and sickness; but he survived the appearance of the tumour more than fifteen years, and finally died, at a tolerably advanced age, of a disease quite unconnected with that on the skull. Mr. Holmes says that "it is obvious on reading Louis's memoir, as well as on studying the specimens of such tumours preserved in our museums, that several different kinds of tumours are included under this designation."

Mr. Holmes, I believe, stands alone in classing a case of *fibrous* tumour of the dura mater with those known as *fungus*. They have nothing in common. Fibrous tumours of the dura mater are exceedingly rare; that mentioned by Mr. Holmes and two recorded by Andral, are all that I can find recorded. In his 'Clinique Médicale,' M. Andral describes a tumour which he found growing from the inner surface of the dura mater, pediculated, and consisting of bundles of fibres closely packed together, in fact, a hyperplasy of the normal tissue of the fibrous layer of the dura mater. Another similar tumour he records as growing in the substance of the tentorium. Rokitansky ('Pathological Anatomy,' vol. iii, p. 115,) says that "in all cases in which fibrous membranes are implicated (by tumours), it is extremely difficult to determine whether the new and the degenerated growths which are imbedded in their tissue were originally formed in them or were developed, as undoubtedly may be proved to be far more frequently the fact, in the parenchymatous organs which those membranes inclose." Now we may regard bones, especially

the flat bones of the skull, which are far more frequently attacked by this disease than any others—for even when others are affected it is always secondarily—as parenchymatous organs surrounded by a fibrous investing membrane; and in the case of fungus tumour of the dura mater, we can readily recognise that it belongs to the organ and not to its investing membrane, because the tumour-substance may always be stripped off the periosteum (either pericranium or dura mater), and although it may and rarely does perforate the fibrous layer, it never involves it. The disease is essentially one of the periosteum. The layer of osteal cells of Tomes and De Morgan exist between the layers of membrane which in after life become the pericranium and the dura mater, and it is a diseased condition of these cells which produces the so-called fungus tumour. During the living existence of the bone this split layer of cells continues its work of renewal. In the diseased condition the cells are abnormally increased in number, size, and shape, and the deposition of bone crumbs does not take place in them, at least, in a normal direction, Thus cell-territories of bone are destroyed layer by layer from the surface, giving the peculiar appearance observed under such circumstances—an appearance which is totally unlike caries. This is strongly supported by the fact that these tumours very frequently contain minute spicules of calcareous matter. It is also very remarkable that they erode the skull whether they grow from the pericranium or the dura mater, or both; and it is not difficult to imagine, from the dissection of the case of Oberhouse, that many instances have owed their origin simultaneously to the pericranium and dura mater, the double erosion causing them to meet and amalgamate, and ultimately pulsate. It is a curious fact, strongly in favour of this, that the tumour outside is generally much larger than the perforation.

This disease seems almost to be peculiar to bones developed from membrane, its appearance in other bones being rare, and always secondary. In one of Lawrence's cases, the disease perforated the sphenoid from below, growing, probably, from the internal pterygoid plate, which is developed from membrane. The same disease in the humerus of the same patient was secondary, and the fracture to which only passing allusion is made, was probably not a fracture in the surgical sense (although actual fracture from very slight cause is common in patients suffering from malignant diseases); it was more probably the margin between the tumour and the sound bone, a solution of continuity having taken place by the slow removal of the bone crumbs and their non-replacement, just as there is no connection between the perforating tumour of the calvarium and the aperture through which it passes. In conclusion, then, these tumours may be regarded as homologous, according to Virchow's recent definitions in his lectures on tumours. They originate in the layers of osteal cells, and are composed of

these cells altered in appearance and degenerate in function. When, together with the altered histological characters the clinical history of the tumours is considered, they ought to be ranked amongst cancerous diseases.

III.—An Inquiry into the Real Nature of Hysteria. By D. De BERDT HOVELL, F.R.C.S.E., &c.

A NAME is said to be sufficiently good which, irrespective of derivation, indicates the thing meant, and at the same time separates and distinguishes it from others. Tried by this standard, the term Hysteria must be admitted to refer to a condition of ill health with which every one is familiar, but which nobody pretends perfectly to understand; which, although very common, seems to set ordinary rules and regulations at defiance, and perplexes the physician quite as much as it vexes and disturbs the patient.

We have often been told that “the rose by any other name would smell as sweet,” but with an altered nomenclature it would no longer retain its recognised place among flowers, and would undoubtedly lose many of its oldest and best associations. Now that which is very undesirable for a beautiful flower like the rose, would be a very advantageous change for a disagreeable morbid condition so much beset with apparent anomalies and unpleasant reminiscences, as the disease miscalled Hysteria, and it would be a great advantage to get rid of that sort of name which given to a dog would hang him.

The ignorance of dark ages has ever led mankind to seek explanation of preternatural phenomena by spiritual agencies. Persons whose condition or conduct was unaccountable were said to be possessed by devils or evil spirits. Those who from circumstances seemed to be endued with more than ordinary power or supernatural influence were accounted wizards or witches, and considered to exercise witchcraft. “The voices” heard by Joan of Arc, which were phenomena incidental to a condition of alternate excitement and consequent depression of nerve-power, were attributed to this evil spiritual influence, and an English Peer, and Roman Catholic Bishop had her burnt accordingly: and in the present day the Rev. Dr. Hook, in his ‘Lives of the Archbishops,’ speaks of the Maid of Kent as having been *hysterical*, but he does not state whence he derives this medical opinion. Again, by our own profession, attacks of epilepsy were associated with the “aura epileptica” which preceded them. Thus it became very natural for the physicians of past ages to attribute the phenomena of hysteria to “vapours from the womb;” in short, many even to the present day have regarded the hysterical patient as a person possessed by some demoniacal influence, and finding no other explanatory solution of the ano-

malous symptoms, and no ready means of getting rid of them, have no doubt at various times longed for the divining power of the exorcist to exclaim with authority, "Hold thy peace and come out of her."

The following extract from the 'Dublin Quarterly Journal,' May, 1869, from the practice of Dr. Halton, is a fair representation of many of the features of this class of case.

"CASE VII.—Miss A. B—, aged thirty-six, unmarried, a teacher of languages; she came under my care in the earlier part of last year. I found her suffering from an attack of acute abdominal pain, with a rapid pulse, flushed face, and some tenderness, but evidently of an *hysterical* character, as it was much less when her attention was otherwise engaged. The attack yielded promptly to draughts containing half a drachm each of chlorodyne, sulphuric ether, and compound spirits of ammonia.

"On visiting her next day, I learned she had been confined to her room for the past eight years, and for the most of that time to her bed. She had been treated by various medical men, but had received no benefit. After a careful examination, I came to the conclusion that all her organs were healthy. Her appetite was pretty fair, and she was very well nourished for one so confined. She complained of weakness in her limbs and back, and some slight leucorrhœa. There were various foods which she averred she could not digest, and others much more indigestible which she assimilated with ease.

"I commenced with a careful course of valerianates, both of iron and quinine, with an occasional bitter infusion; regulating the bowels with aloes and assafoetida, with small doses (one thirtieth of a grain) of strychnia and croton oil, as they were occasionally very obstinate.

"I encouraged her, meantime, to hope for a speedy and favorable issue to the treatment, carefully attending to all her accounts of local pains, applying belladonna and opium plasters when the uneasiness was referred to the chest or back, and hot stupes when the abdomen was affected.

"Under this treatment she was up in a month, and in three from the time of commencing the treatment, she walked five Irish miles somewhat to the surprise of her friends, who had been expecting her death for months.

"She has since (nine months ago) remained perfectly well, and has resumed her profession as a teacher.

"I have since met with a similar case, which promises to turn out as favorably."

The treatment of this case was equally judicious and successful. The symptoms were briefly loss of power, which incapacitated from ordinary duties, acute pain which appears not to have had any fixed seat, and some disturbance of appetite and digestive functions. These were effectually removed by the moral treatment of general encouragement, tonics, narcotics, antispasmodics, and suitable aperients. Notwithstanding that the complaints of pain appear not to have been thoroughly believed in, they were *treated as if they were*

real. Thus one source of vexation and annoyance to the patient, namely the distrust and tacit contradiction of her own statements, was removed and set at rest. Two important questions present themselves, why was the case termed hysterical? Why did not the patient get well sooner? How did it come to pass that various medical men did no good in the course of eight years, and that one with good judgment and common sense effected a cure in three months? Was it that the case was really intractable, or, that the right treatment was not adopted earlier? Was the want of success due to the obstinacy and contumacy of the patient, or was the medical view of the case not in accordance with its real nature? Had the "vapours of the womb," or the more modern idea of "uterine irritation" really anything to do with the matter, or, do these fictions exist only in the imagination of the practitioner?

The successful treatment of the case does not lead to any rational explanation of its nature, or account for its long continuance; on the other hand a question is implied, though not actually raised, as to the reality of some of the symptoms; whether partially, at least, the complaints were not fictitious. It is difficult to prove the physical existence of any symptom, pain for instance, which is transient and fugitive, and we come to the conclusion that some cause of physical disturbance must have existed which was neither tangible nor permanent. The usual conditions of life and some bodily functions were perverted, and this state has not infrequently, and perhaps in this particular instance was actually attributed to morbid imagination, fancy, wilfulness, obstinacy, and deceit; in a word, to *moral* obliquity. We thus arrive at a point at which we admit that physical effects are associated with a moral cause. In the case in question both physical and moral disabilities were eventually removed, for the patient resumed the exercise of her profession as a teacher. It appears then, that, for the space of eight years, some cause prevailed which incapacitated this lady from the exercise of her profession, and during that time her capabilities were suspended. We are thus compelled to recognise and inquire into the influence of psychical, *i.e.* mental and moral action in producing physical disease.

I. On the critical day of the commercial panic in 1866, a man presented a bill of large amount, in Lombard Street. The bill was dishonoured. The man dropped, and died on the spot. It is a point of secondary importance to inquire whether his heart was perfectly sound, or whether it was fatty. The shock produced death. Now, if a cause of this magnitude produce death in a man, it is not difficult to perceive that a shock of less degree would suffice to influence the more delicate organization of a woman, and affect her health in a greater or less degree. To treat such a case physically, and to ignore the psychical element, would necessarily be to treat such a case imperfectly.

II. When, after a long and fatiguing march the British Army was approaching Magdala, many men of the 4th Regiment fell out of the ranks and remained behind, many murmured and were discontented; the order of march was disarranged, disorganization and demoralization had begun. Suddenly the enemy appeared in sight, the bugle sounded, weariness, fatigue, pains, discontent, hunger and thirst were forgotten, all eagerly came to the front, ready for action. Here physical fatigue, together with some sense of hope deferred, and disappointment, sufficed to cause moral depression, which was soon dispelled by "the trumpet call of duty."

III. A labouring man was partially buried by a slip of earth, and nearly an hour elapsed before he could be extricated; for a time he was cold and almost pulseless, but when he was disinterred he asked anxiously for his wife, and became *hysterical*. It is quite clear that this condition was the combined effect of shock and consequent emotion. Its occurrence in a man also proves that the hypothesis which connects the condition with the organ with which the name assumes that it is inseparably connected, is quite fallacious. It is surprising that in this age of enlightenment a term, so wrong, and involving so much ignorance and prejudice, should have been so long retained, and pertinaciously used. It is simply an emotional condition affecting the physical frame; and being in itself psychical, that is to say essentially moral, it affects the moral condition of the patient as well as the physical; the results are not only physical disorder and loss of physical strength, but moral derangement, loss of moral power, and weakening of moral control; moral courage, energy, determination, are alike lowered and depressed; add to these the fact that the condition of depression, whether physical or moral, is one very obnoxious to irritation, and we get at once the key to the explanation of the whole phenomena miscalled hysterical.

It thus appears that the effects of moral shock may be:

I. Primary; acting decisively upon a physical organization which may be sound, or imperfect; or,

II. Secondary; being induced by fatigue or some other form of physical depression; or,

III. Emotion, or moral shock may ensue upon physical shock; the latter acting as a predisposing cause.

We next see that some persons are more easily affected in this manner than others, and this predisposition may be increased by favouring circumstances.¹ Next to the liability to attack we are

¹ James V, of Scotland, died under similar circumstances. Mr. Hosack remarks, "His untimely death furnishes a solitary example of a prince of vigorous intellect, and in the very prime of manhood, sinking under the weight of intolerable grief." "He could die," says Drummond, "but he could not digest a disaster," Robertson's 'History,' p. 285.

bound to consider the power that the constitution may have to throw off the effects; so that we have not only to determine the effects of moral shock and what are the immediate results, but also whether those results are temporary, or more or less permanent; also, what liabilities are left behind. The importance of distinguishing between symptoms of a purely physical nature, and those that have a moral or psychical origin next becomes evident, also, of not attributing to fanciful or superstitious origin, symptoms which have reality in their existence, notwithstanding their apparently unreal and insufficient cause.

The word moral is here used as synonymous with psychical, because, although the Psyche or $\psi\upsilon\chi\eta$ represents the combination of the mental and moral qualities, it is assumed that the moral is the superior, the more powerful of the two; the term moral is frequently used in this sense in relation to the mental and moral qualities.

If we assume, for the sake of argument, that a patient in the condition first described is suffering from the effects of shock of some kind—grief, fear, disappointed hope, broken confidence, or what not, we might reasonably expect prostration of strength, loss of wonted energy and resolution, inability to perform accustomed exercises, &c., or that the exertions of ordinary life, if persevered in with difficulty, and against inclination, should result not only in early and undue fatigue, but in pain, restlessness, malaise, inability to sleep, loss of appetite, &c. If the nature of the case were thus imperfectly understood, or not rightly apprehended, it is very possible that medical advice, under a wrong impression, might urge the patient to undertake a task which she was unequal to attempt and unable to accomplish. It is very possible that the failure of this attempt might be imputed to wilfulness and obstinacy, and the condition of ill health to fancifulness, *because no sufficient reason for the morbid condition would be apparent*. There would be no real psychological difference between such a misinterpretation of the true nature of the case and that which referred the enthusiasm of Joan of Arc to witchcraft, and the idea carried out would, making some allowance for the different historical age, result in similar barbarous treatment.

But to return to the patient. The virtual effect of such misunderstanding would be to add irritation, in the form of worry, to the condition of depressed power already existing; the result would be paroxysms of some kind, spasms, palpitations, muscular contractions, rigidity, state of unconsciousness, and paralysis, miscalled hysterical.

The condition of depressed nerve-power under consideration presents two phases, a tendency to exalted action in the form of preternaturally acute sense of sound, touch, and feeling, constituting a

general state of hyperæsthesia; in this state, if the attention be diverted to other objects, or some matter of interest be presented to the mind of the patient, this condition of over-sensitiveness would be superseded. It is well to bear this in mind, for the condition is open to much misinterpretation. On the other hand, the depressed condition of nervous system is very obnoxious to irritation from both moral and physical causes. I have traced the so-called hysterical paralysis in the case of a schoolgirl to worry, and the tendency to drag the lower limbs in others has been much relieved by unloading the colon and rectum of scybala; in addition to this palliative treatment it is necessary to raise the power and improve the strength of the patient in order to effect a cure. "How is it," writes a patient, "that whenever I am worried the headache and distressing attacks of sickness, of which I have so often complained, invariably come on?" Again, if, on the one hand, physical fatigue conduces to create a susceptibility to emotional influences, physical exercise is, on the other, an essential point of treatment in these cases. So long as the patient lies in bed she is debarred from exercise, which is the great means of keeping up and improving strength; thus the pain and undue fatigue which follows exercise must be superseded by instituting the inducement of some ulterior object; in short, the patient must be soothed, helped, and encouraged; not irritated, bullied, or forced. Viewed through the coloured medium of hysteria, the case is complex, anomalous, incomprehensible; regarded as a condition of depressed nerve-power,—of neurosis, from emotional causes, nothing can be more simple. The physical cause of uterine irritation must not be mistaken for the psychical effect of adverse emotional influence. If worry does not actually cause pain it sometimes tends to make it acute. I attended a young married woman who complained of severe pain in the breast and left hypochondriac region, and her sense of pain was so vivid that it was in vain I assured her that there was no organic disease in those two particular regions; she was not satisfied without going upstairs to undress, and give me the opportunity of having ocular demonstration. I was somewhat at a loss to account for this excessive pain, for otherwise her ailments were slight, until she told me that she had been a good deal worried in a trivial way. Her husband's family would insist that she was pregnant, and she had very good reason for knowing that she was not, and they kept persecuting her to lie on the sofa and keep her feet up, which she did not at all like, and considered to be quite unnecessary. "Very well, then," I said, "do as they wish; when they come to see you, lie on the sofa and put your feet up, and when they have gone away, get up and walk about again." She laughed heartily at the simple stratagem, and I heard nothing more of the pain in the side.

Pain, as well as functional disturbance, is also frequently co-

existent with anxiety, which may be regarded as a graver form of worry. A gentleman about 67 years of age, who was under great anxiety connected with business, suffered from great pain and pericardial distress, which came on regularly at three a.m. He almost invariably woke up from sleep to suffer in this manner, and could never obtain other than very broken rest afterwards. The valvular condition of the heart was sound, but there was reason to suspect structural degeneration. Various remedies were prescribed, and different modes of treatment adopted, with only partial success. The subsequent personal experience of this patient quite coincided with the opinion above mentioned, for meeting me some time afterwards, he exclaimed, "I have lost my anxiety, and I have lost my pain."

There remains yet another view of the condition under investigation, which, although not usually taken, may yet be advantageously considered, namely, the view taken by the patient of the comparative effects of treatment pursued in accordance with the opposite opinions entertained of this class of case. The writings of the poet Cowper, which are remarkable for their truthfulness, simplicity, and purity, contain a record of his own appreciation of the kindly services of a distinguished member of our own profession, rendered to himself under circumstances of painful moral depression. As a matter of course the condition is morbid, or it would not call for medical care and attention.

"Virtuous and faithful Heberden! whose skill
Attempts no task it cannot well fulfil. . . .
"Look where he comes! In this embowered alcove,
Stand close concealed, and see a statue move."

Then follows the personal description of his own sufferings:—

"Lips busy, and eyes fix't, foot falling slow,
Arms hanging idly down, hands clasped below,
Interpret to the *marking eye* distress,
Such as its symptoms can alone express. . . .
This is a sight for pity to peruse,
Till she resemble faintly what she views. . . .
Till sympathy contract a kindred pain. . . .
This, of all maladies that man infest,
Claims most compassion, and receives the least.
Job felt it, when he groaned beneath the rod
And the barbed arrows of a frowning God,
And such emollients as his friends could spare;
Friends, such as his, for modern Jobs prepare.
Blest, rather curst, with hearts that never feel. . . .
And minds that deem derided pain a treat. . . .
"Their sovereign nostrum is a clumsy joke
On pangs enforced by God's severest stroke;
But with a soul that ever felt the sting
Of sorrow, *sorrow is a sacred thing.*"

Next follow valuable hints as to treatment in such cases :—

“ Not to molest, or irritate, or raise
A laugh at his expense”. . . . but
“ He that has not *usurped* the name of man,
Does all, *and deems too little*, what he can
T’ assuage the throbbings of the festered part,
And stanch the bleedings of a broken heart.
’Tis not as heads that never ache suppose—
Forgery of fancy, and a *dream* of woes. . . .
No wounds like those a wounded spirit feels,
No cure for such till God, who makes them, heals.”

So much for the psychological interpretation of this state, *versus* the pathological hypothesis of uterine irritation. The recognition of the physical effects of moral or psychical causes is necessary to the right understanding of the subject. The *modus operandi* of these influences, and the particular portion of the nervous system which they affect, still require investigation and determining. For the present it will be sufficient to remark, that the moral and motor powers are affected more than the mental; the *feelings*, both moral and physical, more than the *intellect*; motion, sensation, volition, together with those portions of the nervous system which direct and control circulation, more than the functions which are strictly cerebral; the spinal marrow and the sympathetic more frequently than the brain.

IV.—The Causes of Insanity in Arctic Countries. By W. LAUDER LINDSAY, M.D., F.R.S.E., Physician to the Murray Royal Institution (for the Insane), Perth.

THE *Etiology* of Insanity in arctic or extreme northern countries is in various respects peculiar or exceptional. In countries such as Greenland, Iceland, and Lapland, there are certain causes directly productive of, or indirectly conducive to, mental disease that either do not exist in more southern latitudes, or, if they are operative at all, operate on a much smaller scale or under very different circumstances of detail. The great peculiarity of the etiology of insanity in high northern latitudes is the dominance of *natural* or external—in other words, non-preventible—circumstances or conditions, necessarily or essentially connected with, or depending upon, *latitude and climate*. These natural influences are productive of a mental habitude or constitution *that belongs not only to individuals, but whole peoples*;—a constitution variously characterised by travellers as melan-

choly,¹ apathy, indifference, and one that is in itself a predisposition to insanity or a stage in its production. Insanity in arctic countries furnishes instructive illustrations (1) of the great variety of circumstances under which the same result—mental disorder—is produced in all races of man in all parts of the world; (2) of the different susceptibility of mind, in civilised as compared with savage races, to be affected by the same disturbing influences; (3) of the equal influence on mind and body of the same causes of depressed or disordered vitality; and (4) of the complexity of causation in insanity. Moreover, the same influences that affect the mental constitution of man there produce madness in certain of the *lower animals*, or a condition which at least has been described as *madness or lunacy* by travellers. It may perhaps appear a paradox to attach the term *insanity* to the affection referred to (for instance, in the dog), implying necessarily as that term does the idea of *mind*. But a consideration of the phenomena of the so-called *madness* of the lower animals and its causation reopens the puzzling question whether and how far these animals are endowed with *mind*; and, if the existence of mind is conceded, how far there is a parallelism as regards mental physiology and pathology between man and the lower animals!

Notwithstanding the great interest that attaches to the study of the causation of insanity in arctic regions, even were it in relation only to the health of the crews of our exploring expeditions and whalers, it does not appear to have attracted notice, or at least adequate notice, among the alienists of this country. I do not find the natural influences above alluded to commented on in any of our works or journals relating either to psychological or general medicine. Nevertheless, the effect of such influences in the production of mental depression or lethargy in whole peoples, and of frequent insanity in individuals, is a common subject of remark by travellers in all high northern latitudes.

The causes of mental disturbance among arctic nations may be conveniently divided into those which are *natural*, depending directly on latitude and climate; and those which are *artificial* or self-produced by man—though the latter frequently, if not generally, are also, indirectly at least, connected with geographical position.

I. *Natural* circumstances or conditions.

1. *Darkness*, or privation of sun-light, in connection especially with the long winter.
2. *Solitude*, or privation of society.
3. *Hunger and thirst*, or privation of food and drink.

¹ Thus the Edinburgh Cabinet Library vol. on 'Iceland, Greenland, and the Faroe Islands,' (*postea cit.*)—the authorship of which is ascribed to Professor Nicol of Aberdeen—speaking of Iceland, refers to "the *melancholy disposition of the nation*," p. 219.

4. *Monotony of the scenery*—its prevalent gloominess—the absence of higher, and especially arboreal, vegetation.

5. *Monotony of occupation*, especially during the long winter.

6. *Cold*, or privation of sun-heat.

7. *Other meteorological conditions*, especially the prevalence of fog or mist, or of rain, sleet or snow.

II. *Artificial or self-produced causes.*

1. Use of putrid, sour, indigestible, or *improper foods*, including especially those which are *salt* or *fat* ; or,

2. Restriction as regards *variety of food*—exclusive use of a certain kind (such as salt or fat *fish*), with *privation of fresh vegetables*.

3. *Intemperance* in the use of coarse, strong, spirituous liquors.

4. Inattention to *cleanliness of skin*—want of ablution and bathing.

5. Insanitary conditions of *dwellings*, especially as to ventilation : excessive heat in relation to the external temperature : and the generation of noxious effluvia from decomposing animal matters.

With the exception of intemperance, these artificial causes of insanity operate generally *indirectly* by the production of general ill-health, or of specific affections such as leprosy or scurvy. Intemperance, as in more southern countries, is itself a frequent immediate or exciting cause of insanity, though its influence as a cause, whether exciting or predisposing, is I believe, in arctic as in other countries, greatly over-rated.¹ Though an arrangement of convenience, which brings into due prominence certain exceptional causes of insanity in arctic countries, it is obvious that a classification which dissociates the natural from the artificial influences is itself highly *artificial* and unscientific. There is an inseparable connection between the said natural and artificial causes. It is obvious that the natural surroundings lead to the peculiar habits of life as regards dwelling and food ; and even intemperance may be regarded as the to-be-expected product of the ennui resulting from the combined influence of protracted darkness, intense cold, solitude, gloomy scenery, and hunger. It is too frequently impossible to distinguish *predisposing* from *exciting* causes, or to *isolate* certain influences as factors of mental disease. Generally, if not always, the causation of insanity is complex in its character and slow in its operation. The natural or necessary interdependence of the several influences above enumerated as factors of insanity will, however, more clearly appear in the sequel.

The operation of these influences may be considered as they variously or severally affect—

¹ I pointed this out in 1858, in the 31st Annual Medical Report of the Murray Royal Institution, p. 28.

(a) The *natives* of arctic countries, such as the Eskimo, Laplanders, or Icelanders.¹

(b) *Colonists* or temporary residents, such as the Danes in Greenland and Iceland.

(c) *Visitors*, or very temporary residents, such as the members of exploring expeditions or the crews of whalers.

(d) The *domesticated animals*, especially the dog.

To discuss fully the operation of all the influences already mentioned on these several classes of man and animals, would require a somewhat lengthy treatise. All that I propose at present is to offer such *illustrations* merely of the operation of the influences in question as may serve to indicate their importance in reference to mental pathology. The observations I have to submit are based partly on personal inquiry in Iceland and Norway; partly on the narratives of arctic explorers or travellers; and partly on works relating to medical topography drawn up by Danish physicians sent as commissioners by their Government to investigate and report upon the special diseases of such countries as Greenland, Iceland, or Faroe, and their causation. I visited Iceland in June, 1860, spent a week in Reykjavik, its capital, and there had frequent interviews with the most intelligent and experienced Physician-general of the island, Dr. Jón Hjaltalin. To him I am indebted for much valuable information relating to the peculiar diseases of his countrymen. I had also an opportunity, partly by his kind invitation, of seeing for myself some of these diseases, including leprosy,² hydatid disease of the liver, and insanity (melancholia). On the voyage to and from Iceland I called at Thorshavn, Faroe. Norway was visited in August, 1857. I spent some days in Christiania, and there had interviews with Dr. Sandberg, the Director of the Gaustad Asylum, Drs. Winge and Kaiser, of the City Asylum at Mangelsgaarden, and with Professors Holst and Boeck, of the University, and General Hospital, of Christiania. To these gentlemen, and to the documents which they placed

¹ I am quite aware that, in one sense, some at least of these peoples may be regarded rather as *colonists* or *immigrants*. Thus ethnologists have shown the probability of a migration of the Eskimo from Asia; and there can be no doubt that the Icelanders came originally from Scandinavia, especially from Norway. But for all practical purposes—and especially for our present purpose—all these peoples may be considered more thoroughly *native* than at least the present native-born Americans (of the United States) or Canadians.

² According to Dr. Hjaltalin, it occurs in two forms—*Elephantiasis anæsthetica* and *E. tuberculosa*. It is the "*Likthra*," "*Holdsveike*," or "*Spitelska*," of Icelandic authors (=the genuine *Elephantiasis Arabum*), believed to have been introduced from Europe or the East at an early period in the colonisation of Iceland. But little is really known of its early history in Iceland. It is also the "*Spedalske*" or "*Spedalskhed*" (= *Eléphantiasis des Grecs*) of Scandinavian writers, e. g. Danielsen of Bergen, and Boeck of Christiania, Norway. The Chevalier Bach of Stockholm, Sweden, improperly calls it "*Icelandic Scurvy*."

in my hands, I was indebted for full information regarding insanity in Norway.¹

1. *Darkness, or privation of sun-light.*—"Of all the discomforts," says a reviewer in the 'British and Foreign Medico-Chirurgical Review,'² "attendant upon wintering within the arctic circle, none, perhaps, is felt to be so depressing as the *absence of light*. . . This continued night is particularly injurious to the *mind*. The temper becomes irritable, the intellectual energies impaired, and the habits of some become gloomy and solitary," (p. 18). McDougall,³ the arctic navigator, writes (p. 277), "We have now before our eyes, in the crew of the 'Investigator,' living proofs of the injurious effects of a prolonged stay in these regions on the *mental as well as the bodily* capabilities of the human frame. . . . With the approach of the second winter in the Bay (the third within the arctic circle) an apathetic indifference pervaded the men. With their strength, which had decreased with the continuance of the small allowance of provisions, all their energy seemed to forsake them, and as symptoms of scurvy were manifested in the system, they lost all animation, and allowed a feeling of lethargy to master them, which utterly precluded any hope of success in hunting. . . . The above facts, in my opinion, tend to prove that, even amidst comparative plenty so far as *animal* life is concerned, the approach of a third season brings with it a deep *depression of spirits*, which few minds are strong enough to bear up against. . . . With the sun, as he sinks beneath the horizon, leaving the dark and gloomy night of three months' duration to usurp his throne, the last ray of hope departs." Though McDougall here apparently attributes the mental depression of his crew to the winter *darkness*, it is at least equally legitimate to attribute it to *scurvy*,⁴ or the causes that produced that debilitating disease. Dr. Kane, the distinguished American arctic explorer, remarks,⁵ "I feel that we are fighting the battle of life at disadvantage, and that an arctic night and an arctic day *age* a man more

¹ A full report "On Insanity and Lunatic Asylums in Norway" was published in Dr. Winslow's 'Psychological Journal' for April, 1858.

² For January, 1861, in an able article on "Man and Climate," to which I confess myself largely indebted for references.

³ 'The Voyage of H.M. Discovery Ship "Resolute" to the Arctic Regions in search of Sir John Franklin and the missing Crews of H.M. Ships "Erebus" and "Terror," in 1852-3-4.' By Geo. F. McDougall, Master. London, 1857.

⁴ The connection here between the development of scurvy and the want of *fresh vegetables* (for fresh *animal* food was abundant) is instructive. The same connection has been frequently observed in Iceland, where scurvy is of the ordinary kind—that (viz.) which is most familiar among sailors on long voyages. It is developed chiefly in periods of scarcity of fresh food, as after or during long or severe winters; and the great remedy is fresh vegetable food, and varied, as well as fresh, animal food; some native plants (e. g. *Cochlearia officinalis* and various *Rumices*) being occasionally used as substitutes for potatoes, cabbage, and onions.

⁵ 'Arctic Explorations in the Years 1853-4-5:' being a Narrative of the Second

rapidly and harshly than a year anywhere else in all this weary world," (vol. i, p. 173). Here, again, it is impossible to dissociate the several influences of darkness, cold, solitude, &c. Even in countries or localities situated far south of the arctic circle the winter darkness leaves its impress on the feelings, whether of natives or sojourners. Thus, speaking of "Winter Life in St. Petersburg," the distinguished American traveller, Bayard Taylor, remarks¹ (vol. i, p. 108), "the *physical depression* we all felt seemed to result chiefly from the *absence of daylight*." Here the other influences that co-operate with darkness in the production of mental depression in high latitudes could not have existed. Cold no doubt did co-exist, but this does not seem to be so influential a cause of mental depression as solitude, monotonous scenery, or uniformity of occupation. A reviewer of 'Dr. Kane's Arctic Explorations,' in the 'North British Review,'² writes, "The influence of this long and intense *darkness* was depressing to the crew; and even the *dogs*, though born within the arctic circle, were unable to withstand it. . . . Dr. Kane believed that the strange disease (in the latter) was a *mental* affection, originating in darkness," (p. 421). Again, "Dr. Kane observed that the darkness of the long winter nights had a fatal influence upon his *dogs*. A disease, which he considered *clearly mental*, affected to such a degree . . . his Newfoundland team, that for a fortnight they were doctored and 'nursed like babies.' They ate and slept well and were strong, but an *epileptic* attack was followed by *true lunacy*. They barked frenziedly at nothing, walked anxiously in curved lines, at one time in moody silence, at another starting off howling as if pursued, and running up and down for hours. They generally died with symptoms resembling locked-jaw, in less than thirty-six hours. Three splendid Newfoundlanders and thirty-five Esquimaux dogs thus perished, and only six of the whole pack survived. At a future time, one of Dr. Kane's best dogs was seized with a similar disease, and, in the delirium which followed his seizure, 'he ran into the water and drowned himself, like a sailor with the horrors,'" (p. 413). Similar phenomena, popularly ascribed to *madness*, are familiar as occurring in various domesticated animals in all parts of the world. In our own country the so-called madness of the dog, cow, or bull, is developed during the heats of summer. The celebrated navigator, Captain Cook, described "fits bordering on madness" as leading to the suicide by drowning of certain goats he destined for colonial acclimatisation, the cause in them apparently being the poisonous action on the brain of the narcotico-irritant Toot plant of New Zealand, (p. 154). I have also myself described

Grinnell Expedition. By Elisha Kent Kane, M.D., U.S.N. 2 vols., Philadelphia, 1856.

¹ 'Byeways of Europe.' London, 1869.

² For February, 1857.

the madness of sheep and cattle produced by the action of the same Toot poison (pp. 166 and 168).¹ In all cases of madness in the lower animals the *brain*, and perhaps, also, less or more, the general nervous system are involved; but whether, and to what extent, *mind* is implied or involved, I cannot stop here to inquire, though the subject is one of much psychological interest.² Several cases of insanity occurred in Dr. Kane's crew. For instance, he mentions Ohlsen, after enduring great privations, "who had been fifty hours without rest, fell asleep, and awoke with unequivocal symptoms of mental alienation," (p. 422). But in all these cases of Dr. Kane's sailors, hunger and cold, fatigue and want of sleep co-existed with darkness as factors of mental disorder.

Darkness, as a cause of insanity, or of mental deterioration, is not confined to arctic countries. It may be found operating, under different circumstances, however, and on a greatly less scale, in the most highly civilised countries, and in the most populous cities, of the south. Illustrations occur in the mental etiolation that results among political and military prisoners from dungeon life in some of the despotic countries of Europe, or even among the lower orders of our own large towns from confinement to dark streets, alleys, or courts. In these cases, however, the darkness is *artificial and preventible*.

2. *Cold, lowness of temperature, or the privation of sun-heat in relation no less to its duration than to its intensity* during the long arctic winter. The depression of vitality produced by cold is well known, even in our own country, where low temperature, especially when sudden in its advent, leads directly or indirectly to a large mortality every winter. Madame D'Annet speaks of the physical, and mental or moral, lethargy of the Lapps as attributable to the *coldness* of their climate;³ and Bayard Taylor mentions in a similar way their "patience, indifference, apathy."⁴ Here again, however, it is impossible to dissociate the influence of cold from the co-operating action of darkness and the other peculiarities of arctic countries or peoples already, or hereinafter to be, commented on. Under this head note may be taken of the sudden transitions of temperature to which the inhabitants of arctic countries subject themselves by the excessive heating *without ventilation* of their houses or huts; as well as of the other meteorological conditions⁴ that co-operate with cold in

¹ "On the Toot Plant and Poison of New Zealand," 'British and Foreign Medico-Chirurgical Review,' July, 1865.

² To the subject of *Insanity in the Lower Animals* I purpose devoting a separate paper.

³ 'Voyage d'une Femme au Spitzberg.' By Madame Léonie D'Annet. Second edition, Paris, 1855.

⁴ 'Northern Travel: Summer and Winter Pictures of Sweden, Lapland, and Norway.' London, 1858.

⁵ I pointed out some of the relations of *meteorological or climatic conditions* to

the production of physical debility or disorder, such as the persistent superabundant moisture of the atmosphere.

3. *Monotony and gloomy character of the scenery.* The great peculiarity of the arctic landscape is the usual absence of forests or trees. Arctic countries consist mainly either of snow-fields and glaciers; of barren mountain ranges of unvarying hue; of even more sterile lava fields or wastes of volcanic sand, ash or pumice; or of stony deserts. These physical phenomena are illustrated respectively by Greenland, Iceland, Lapland, and the Samoyede country.¹ In countries which are less arctic and less devoid of the higher forms of vegetation, forests themselves are quite as monotonous when they consist of one tree or group of trees, such as the firs or pines, and cover large tracts of country. The writer of "An Historical and Descriptive Account of Iceland, Greenland, and the Faroe Islands"² remarks, speaking of Iceland (p. 189), "The melancholy character of the climate and scenery, together with the remembrance of the faded glories of their country, has given a peculiar impress to the *minds of the people*. Dwelling in desolate places, deprived of almost all vegetation, in dark miserable houses, where the light of day can scarcely penetrate, amid scorched rocks of rugged lava, or enclosed between the raging sea and the black cliffs, they become serious, quiet, humble, and little disposed to exert themselves, unless impelled by necessity." Dr. Panum³ attributes the prevalent insanity of Faroe to the gloomy character of the scenery; but he goes on to point out that this gloom is added to by the *constant mists*. He also shows the great contrast that exists between the summer and winter life of the Faroese. In summer it is extremely active, their chief occupations being whale-hunting, sea-fishing, rock-fowling, or tilling the scanty soil. There is no night proper, and no interruption to their labour. In winter, on the other hand, there are only a few hours of daylight. The people are often cut off for weeks, by storms, from their customary occupations, or from any muscular exercise, as well as from their fellows. It is obvious that the mere character of *scenery* can never operate *by itself* as a factor of mental disturbance. It cannot, *e. g.*, be separated

the development of insanity in the following Annual Medical Reports of the Murray Royal Institution, viz. 31st, p. 27; 33rd, p. 47; and 34th, pp. 25 and 46.

¹ I have sketched some of the general features of the vegetation of Iceland, Faroe, and Norway, in a paper on the "Lichen-Flora of Northern Europe" ('Journal of the Linnean Society, "Botany,"' vol. ix, p. 402); of Greenland, in my "Lichen-Flora of Greenland" ('Transactions of the Botanical Society of Edinburgh,' vol. x, p. 35); and of Iceland, in my "Flora of Iceland" ('Edinburgh New Philosophical Journal,' July, 1861, p. 22).

² Volume of the 'Edinburgh Cabinet Library.' Edinburgh, 1840.

³ 'A Narrative of the Cruise of the Yacht "Maria" among the Faroe Islands in the Summer of 1854' (authorship ascribed to Mr. Greig). London, 1855. Dr. Panum was sent in 1846 by the Danish government to draw up a Medical Topography of Faroe, and his resultant 'Medical Statistics of Faroe' are commented on in the 'Medico-Chirurgical Review,' vol. vii, 1851.

from another powerful influence, which is almost invariably found acting in association, viz. solitude. Conjunctly, however, these influences are undoubtedly frequent causes of mental depression or disease in all parts of the world; for instance, in our colonies¹ and even in our own Scotland,² in classes of men peculiarly exposed by the nature of their occupations there to such, as shepherds. A parallel to the monotony or gloom of natural scenery in all wild countries is to be found in that *uniformity of surroundings* which characterises the dwellings of the poor in the most densely built portions of all our great towns,—a monotony of street and house-structure that to many minds would be infinitely more damaging than any uniformity in the natural landscape!

4. *Solitude, or privation of society.* The well-known essayist of “Fraser’s Magazine,”³ A. K. H. B.,⁴ in one of his attractive essays, “Concerning solitary days,” points out so truthfully the effects of solitude on the mental organization of man that I need offer no apology for here making a few quotations. “There can be no question at all that in a solitary life *nature* rises into a real companion, producing upon our present mood a real effect” (p. 219). “Another consequence of a lonely life in the case of many men is an extreme sensitiveness to impressions from external nature. In the absence of other companions of a more energetic character, the scenes amid which you live produce an effect on you which they would fail to produce if you were surrounded by human friends (p. 218). The solitary man is apt to look too much inward, and to attach undue importance to the fancies and emotions which arise spontaneously within his own breast, many of them in great measure the result of material causes (p. 219) Let us try to be entire human beings and if we wish to be so it is best not to live too much alone. The best that is in man’s nature, taken as a whole, is brought out by the society of his kind. In one or two respects he may be better in solitude, but not as the complete man” (p. 222)—whose highest possession, it may be added, is or ought to be the *mens sana in corpore sano*! Dr. Armstrong, surgeon and naturalist of H.M. exploring ship the “Investigator,”⁵ writes:

¹ Insanity resulting from the *solitude* of a shepherd’s life in New Zealand is illustrated in a paper on “Insanity in British *Emigrants* of the Middle and Upper Ranks,” in the ‘Edinburgh Medical Journal’ for September, 1869, p. 218. In quoting this paper it is necessary to explain that, on account of its having been printed without revision of “proof” by me, it contains numerous typographical errors, some of them quite destructive of the sense!

² 32nd Annual Medical Report of the Murray Royal Institution, p. 9.

³ For February, 1861.

⁴ It is no secret that this is the Rev. Dr. Boyd, of St. Andrew’s.

⁵ ‘A Personal Narrative of the Discovery of the North-west Passage; with numerous Incidents of Travel and Adventure during nearly five years’ continuous Service in the Arctic Regions, while in Search of the Expedition under Sir John Franklin.’ By Alex. Armstrong, M.D., R.N. London, 1857.

"It was generally remarked how vacant was the stare and how fatuous and inexpressive their countenance when contrasted with healthy men, by those unaccustomed to view such objects as the "Investigators" then presented, thus affording truthful evidence of the shock which the intellectual faculties had sustained, and the mental prostration that ensued after so long a period of complete *isolation from the world*, under trying circumstances" (p. 570). But these "trying circumstances" were probably quite as potential as isolation or solitude in the causation of mental deterioration, including, as they must have done, darkness and cold, or other conditions peculiar to arctic exploration! Solitude, as a cause of insanity, is by no means confined to the thinly-peopled wilds of northern countries, or to the shepherds of our own Highland valleys. It is frequently operative among all classes of society in all our centres of civilization. I have had many patients in whom insanity could not be ascribed to any other cause than *self-seclusion from society*—living too much alone. Hermits of this class have come to me not only from quiet country districts or villages, but from the very centres of populous cities. They are to be found indeed in *all* asylums for the insane. A further, and perhaps even more instructive illustration may be found in some of the fruits of what is known as the Silent or Separate system of prison discipline in this and other countries.

5. *Food, in relation to its quantity, quality, and variety.*¹ Absolute privation of food—hunger and thirst—are in themselves very rare causes of mental disease. When they operate at all, it is usually in the case (*e.g.*) of cast-away sailors, or lost explorers, in whom we cannot dissociate the conjoint influence of the dread of impending death, or of cold, fatigue, and other privations. Food, however, that is improper either as regards its quality or variety, *e.g.* the exclusive use of salted meat² or fish, *with privation of fresh vegetables*, leads to a series of physical diseases, which frequently culminate in insanity. The food-habits of the Icelanders may be taken as a type of those of the inhabitants of other arctic countries. In Iceland the peasantry live to a great extent on *fish*, mostly salted, frequently fat, and often also putrid. They are largely engaged in the sea fisheries for the supply mostly of southern markets: the fish chiefly caught and cured for export, as well as for home use, being cod,³

¹ The importance of *variety* in relation to diet is shown in the 36th Medical Report of the Murray Royal Institution, p. 56; in which report, also, the whole subject of *diet* in relation to disease is discussed *ad longum*, pp. 28 et seq.

² A uniform testimony is borne by Arctic navigators—and indeed by all navigators—to the evils of living too much on *salted* meats.

³ Fresh *cod* is the "Torsk" of the Danes. Cod cured *without* salt—dried simply in the open air = "Hardur-fiske" or "Torfiske" of the Icelanders; cured *with* salt = "Klipfisk" of the Danes.

(*Morrhua vulgaris*), Ling (*Lota molva*),¹ and the ugly Lump-sucker or Lump-fish² (*Cyclopterus lumpus*).³ All the best fish or the best portions of the fish are exported to Spain (Barcelona and Bilboa), Portugal (Lisbon), or Italy (Leghorn). The least valuable, those that are consequently the least nutritive, they reserve for home use, *e.g.* cod-heads. I remember being struck in and around Reykjavik with the immense quantities of comparatively fleshless fish-heads hung on strings to dry, apart from the fleshy bodies; and the explanation given to me was that the former were intended for winter use of the inhabitants of Iceland itself, the latter for export to foreign markets. The Icelanders occasionally eat skate and other *fresh* or salted sea fish: but on the other hand, they also consume fish-liver and roe and other parts of sea fishes, in a state of *putridity*, besides occasionally in winter the fat and oil of whales, and in summer *sour* milk or whey. The use of the latter, if it is to be had at all, however, is confined to the short summer: while at all times there is a most meagre consumption of *bread and vegetables*. Fresh-water fish are also eaten occasionally: but Dr. Hjaltalin assured me that in localities where the inhabitants live largely on *fat* fish such as cod or lake trout, *leprosy* is more prevalent than elsewhere. It is difficult, however, to believe that in such cases the use of such excellent food is in itself productive of so loathsome a disease. In such cases there are probably also operating as associated influences restriction to the same food, the want of fresh vegetables, and all the disadvantages connected with insanitary habits as to personal cleanliness, and the character of their dwellings. The fact that *scurvy* (as well as leprosy) is one of the most characteristic diseases of Iceland points to improper *restriction to certain articles of food* and to the *insufficiency or privation of fresh vegetable diet*: while the still greater prevalence of *dyspepsia*,⁴ which is as general as it is said

¹ Known as "Stock-fish" when dried for market: a term apparently applied also to *Cod* similarly prepared.

² The "Cockpaddle" of Scotland. This was apparently the chief sea-fish used as food about Reykjavik at the time of my visit.

³ The natives give it different names according to its sex, the male being called "*Randmagé*," because it has a red belly; the female, "*Grasleppa*," because it is grey below. Hooker describes the mode of curing it in his 'Journal of a Tour in Iceland in the Summer of 1809' (2nd ed., 2 vols., London, 1813, p. 231). It is, he says, the most hideous of all fish. The sucker, which is on the under side, is cut away, with a considerable portion of the skin of the belly and the entrails, which latter are used for bait. The remainder is hung up to dry.

⁴ Regarding the *diseases* of the Icelanders the reader may consult (in addition to the standard work of Dr. Schleisner) the following essays:

1. 'Dissertatio medica inauguralis de Morbis Islandiæ.' By Sir Henry Holland, then President of the Royal Medical Society of Edinburgh. Edinburgh, 1811. The same information is given in the chapter by him "On the Diseases of Iceland," in Sir George Mackenzie's *Travels in Iceland*, p. 411.

2. "On the Causes of the Diseases prevailing in Iceland." By Dr. Jón Petursson, in the *Trans. of the 1st Literary Society of Iceland*.

3. 'On the Diseases of Iceland.' By Dr. Svein Paulson, written about the year 1800.

to be in America, is connected usually with the use of food unsuitable as to kind and cooking. As in all arctic countries, *famines*¹ are common, caused for instance by long and severe winters, by failure of the fisheries, or murrain among land animals (sheep or cattle). On such occasions the inhabitants are reduced to great straits, being compelled sometimes to live mainly or exclusively on certain *lichens*, which are collectable at all seasons on their otherwise barren rocks. Of these lichens the chief is the well-known "Iceland moss" (*Cetraria Islandica*)² of commerce, the "Fjallagrös"³ of the native. But Olafssen and Povelsen, who spent two years traversing the island, and who described its natural productions in 1772, mention three other species as being occasionally eaten, viz.: *Umbilicaria proboscidea* L.; *Cetraria nivalis* L.—the "Maria graus" (the Virgin Mary's grass) of the natives, which is said to be extremely agreeable food of a sweet taste;—and the "*Coralloides tenuissimum nigricans*" of Dillenius. Sir Wm. Hooker⁴ mentions also *U. cylindrica* L. as being used in times of scarcity of food; and of the whole genus *Umbilicaria*, Paulsen remarks, "*Longè optimum in re cibariâ Lichenis genus.*"⁴ The effect of some of these lichens on the human system may be illustrated by the case of the *Umbilicarias*—the "Tripe de roche" of the Canadian voyageurs—used by Sir John Franklin, Sir John Richardson, Admiral Back, and Lieutenant Hood, during their well-known expedition down the Coppermine river in Arctic America in 1819-20. When preserved meats, and all game, fish, and other food had failed, they were reduced to living almost exclusively on various species of *Umbilicaria*. By reason of the diarrhœa produced, their state of extreme physical debility was still further impaired, and they were reduced to the condition of skeletons both as regards strength and appearance. The whole history of their life at Fort Enterprise in November, 1820, is harrowing in the extreme.⁵ On the other hand, some of the lichens before mentioned, and especially the "Iceland moss," by reason of their amylaceous and other constituents, would appear to be occasionally invaluable to the natives as *substitutes for fresh vegetables* of a higher kind.

¹ Between 1753 and 1759 *famine* destroyed in Iceland no less than 10,000 persons (Sir Geo. Mackenzie).

² The nutritive and medicinal properties of this lichen are described in my paper on the "Northern Lichen-Flora," p. 411; and in the same paper, p. 413, other food-lichens—used in times of famine in other northern countries—are enumerated. The latter are also mentioned in a paper on the "Arctic Cladoniæ" ('Trans. Botanical Society of Edinburgh,' vol. ix, p. 183); and in that on the "Greenland Lichen-Flora," p. 55.

³ The *Fjallagrös* Harvest in Iceland, with the uses and habitats of the lichen, is described in Nicol's 'Iceland,' pp. 198, 384, and 387.

⁴ In Sir George Mackenzie's 'Travels in Iceland.' Edinburgh, 1811, pp. 417 and 431.

⁵ *Vide* also paper on the "Greenland Lichen-Flora," p. 54.

6. *Intemperance—the abuse of alcoholic liquors.*—Taking, again, Iceland as a type of arctic countries, it has to be observed, as regards intemperance, that spirits are there both cheap, and coarse or strong. The chief spirits used in Iceland, Faroe, and Norway, are either those that are home-made from potatoes or various kinds of grain, which are generally very impure from admixture of fusel oil or other noxious ingredients; or foreign brandies or rum of the commoner kinds. When I visited Reykjavik, Danish (corn) brandy or spirit was 1s. 9d. per gallon, and 6000 to 10,000 barrels per annum were imported, there being 160 bottles in each barrel; that is, about 3d. per bottle. The better qualities fetched 7d. per bottle. Cognac was 1s. per bottle, about 100 barrels per annum being imported. Rum varied from 8d. to 9d. per bottle, 200 barrels per annum being imported. French wine (St. Julien) was 1s. to 1s. 6d. per bottle, while port was 3s., and sherry 2s. 6d. The better French brandies were 2s. a bottle or 5s. 6d. per gallon. All wines and spirits are duty-free, and their cost is mainly the cost of carriage. So much brandy of one kind or other is consumed in Iceland, that, as Dr. Hjaltalin informed me, a duty of only 6d. per bottle would yield a revenue of about 60,000 dollars. It is not then surprising that intemperance is common¹ in extreme northern countries, nor that it is frequently directly productive of insanity, as it is in more southern latitudes.² But in the north, as in the south, it is obviously too much regarded as a *cause* of insanity rather than itself as one of the *results* of that combination of natural influences, which sometimes culminate in insanity. In connection with the drinking customs of northern nations, we must bear in mind the ennui or mental depression produced by protracted darkness, intense cold, gloominess of scenery and weather, uniformity of occupation, solitude, and other influences, any one of which is quite as competent as mere intemperance to induce insanity. Stimulants become almost a necessity of existence to counteract the mental and bodily lethargy or apathy produced by so formidable a combination of external and non-preventible influences. I am assured that in countries infinitely more favoured as to climate, and among people better circumstanced as to all creature comforts, intemperance is a *more apparent than real cause of insanity*. Thus, Major Blewitt, of the 65th regiment, informed me (in 1862), that insanity is not uncommon among the imperial troops in New Zealand in peace times. Such insanity is

¹ Dr. Panum describes confirmed intemperance as rare in Faroe, but says that those who *are* intemperate generally become insane ('Cruise of Yacht "Maria,"' p. 25).

² Among the inmates of the Christiania Asylum at Gaustad, intemperance stands fourth in the list of the causes of insanity, a greater number of cases being attributed to (1) hereditary transmission, (2) onanism, and (3) disappointment in love. (*Vide* paper on "Insanity in Norway," p. 19.)

usually attributed to intemperance. But he ascribed it rather to the ennui resulting from a monotonous life; which ennui, however, is naturally productive of intemperance as well as insanity, or of insanity by means of intemperance as the mere last or exciting cause.¹ To discover the ultimate or predisposing cause, we must fall back upon the influences that produce this *ennui* or mental depression. The causes are usually several and complicated.

7. *Habits of life in relation especially to dwellings, personal cleanliness, and occupations.*—Among the characteristics of the houses of the inhabitants of arctic countries are the following: They are of one story, very thick-walled, whether the walls be of stone, turf, clay, or other material. The floor is sunk in the earth and is frequently of clay. Windows and doors are reduced to a minimum. Frequently there is no chimney, the smoke arising from a central fire escaping by the door when open. All chinks and crevices are carefully closed so as to exclude both moisture and air.² Frequently the interior is the common habitation of man and his domestic animals. Decomposing animal matters, often of the most disgusting kind, are frequently heaped up in front of, or around, and even within, the dwelling; the interior of which is over-heated and is devoid of ventilation. When occupied by a crowded population, whether consisting exclusively of human beings or not, the cubic breathing space—of an atmosphere vitiated in addition by the effluvia of dirty skins—is extremely limited,³ and would be much too little for health even were the air pure and constantly renewed. The ‘Edinburgh Cabinet Library’ compiler writes (p. 201), “To a stranger the *filth and smell* are the most disagreeable accompaniments of an Icelandic habitation, and contribute not a little to the unhealthiness of the inmates.” A parallel to the huts of the Icelandic peasantry, especially of the poorer or fisher class, may be found in the cabins of our own Hebridean Islanders, as I saw them in Lewis in May, 1866. It would appear that peat or peat-“reek” (or smoke) neutralizes to a great extent the noxious atmosphere of the very primitive dwellings of our own Highlanders and Islanders; and it may exercise a similar influence in those arctic countries in which peat occurs.⁴

¹ It is notorious, moreover, that this same ennui is the not unfrequent cause of *suicide* among soldiers.

² Plans and drawings of *Icelandic houses* are to be found in the following works:

1. ‘Island undersøgt fra et lægevidenskabeligt Synspunkt.’ By Dr. Schleisner. Copenhagen, 1849, p. 118.

2. ‘Reise nach Island im sommer 1860.’ By Prof. Zirkel of Kiel, and Dr. Preyer of Berlin. Leipzig, 1862, p. 108.

³ The relation of *Ochlesis*—the huddling together of large groups of dirty human beings in an equally filthy limited area—is familiar in the history of such diseases as typhus and cholera.

⁴ Thus, Dr. Morgan, in a paper “On the Non-prevalence of Pulmonary Consumption in the Hebrides and along the North-western Coast of Scotland”

Like our own Islanders and all arctic peoples, the Icelanders are singularly inattentive to *personal cleanliness*. With abundance of natural warm baths in the form of the hot springs which are scattered over the country, they appear never to dream of bathing, nor does ordinary ablution seem to meet with ordinary attention. Even in Reykjavik no application has been made of the neighbouring hot spring of Laugarness¹ to the purposes of either private or public bathing. Nor does sea-bathing appear to be practised. During my residence in Reykjavik I was consulted in a great variety of the diseases prevalent in Iceland; and having reference to the uncleanly skins of patients of all ranks, my standing prescription was in the first place a course of *warm* bathing, with the plentiful use of *soap*, followed by a course of *cold* sea-bathing. So simple a prescription created intense and general surprise among a people apparently accustomed to a copious use of drugs as a remedy for all the ills their flesh is heir to, and who evidently had sanguine anticipations of the application to their cases of the newest articles of the Pharmacopœias of England or France. A similar regret as to the non-use by the Icelanders for bathing purposes of the numerous hot springs at their command has been expressed by Gaimard,² who writes (p. 34), “Les eaux de Laugarnes dégagent en outre un peu d’Hydrogène sulfuré; et il est bien à regretter que les habitants de Reykjavik ne tirent pas meilleur parti de cette propriété dans un pays où les affections cutanées sont si communes. Avec très peu de dépenses on pourrait assurément en faire des Bains à la fois agréables et utiles. Pour cela je conseillerais de faire passer l’eau des sources tout simplement dans un laugar couvert où se rendrait un autre courant d’eau froide, afin de réduire le Bain à une température convenable. Les eaux, en déposant de la silice, ne tarderaient pas à cimenter elles-mêmes le bassin qu’on y aurait creusé pour l’usage des Baigneurs.” As a contrast to present neglect the statement of Humboldt³ may be quoted that, on Christianity being introduced into Iceland, the natives refused to be baptized in any but the water of the Geysers! Dr. Hooker, in his ‘Himalayan Journals,’⁴ repeatedly mentions the fondness of the natives of the Himalayas for *their* hot springs, in the water of which they will remain for three days at a time, leaving the bath only occasionally, and

(‘Brit. and For. Medico-Chirurgical Review,’ Oct., 1860, p. 483), ascribes this immunity from phthisis to the inhalation of peat-smoke. It is supposed to act by virtue of its tar, creosote, or tannin.

¹ An analysis of its water, by Professor Murray Thomson, now of Roorkee, Bengal, is given in my paper on the “Kötlugja Volcano, Iceland,” in the ‘Edinburgh New Philosophical Journal’ for Jan., 1861, p. 18.

² ‘Voyage en Islande et au Groënlande exécuté pendant les années 1835 et 1836 sur la Corvette “La Recherche.”’ Paris, 1840.

³ ‘Personal Narrative,’ vol. iv, p. 195.

⁴ Vol. i, p. 305, and vol. ii, pp. 117 and 185.

for short periods. The same custom has been noticed by Humboldt in other parts of the world.

The *monotony or uniformity of the occupations*, especially of the men, during the long dreary winters, the want of sufficient physical exercise, undoubtedly form important elements in the production of that ennui or languor, that mental depression or indifference, that physical inertia which is by concurrent testimony the common characteristic of all arctic peoples. For many weeks at a time there is frequently *enforced muscular or physical idleness*. Men, as well as women, are confined to the house by snow or storm; and we have already seen that the close, moist atmosphere of their huts, charged as it is with carbonic acid, ammonia, and other deleterious gases is in itself sufficient to act as a powerful depressant alike of physical and mental vigour. It is fortunate for the better classes of the Icelandic peasantry that they have a strong literary tendency. Indeed, I know of no other people that, as a people, possesses the same literary acquirements and tastes.¹ To the cultivation of these tastes they devote the long nights of the long winter, and are thereby refreshed and ready for the out-door occupations,—the fishing or hunting, the pastoral or agricultural pursuits, that characterise or constitute their summer life. But far different is the case of men who have *no mental resources*, and who, if deprived of the mental excitement which they find in the pursuits by which they live—for the most part fishing—are naturally led or driven to the brandy bottle.²

The *form* of insanity, which is most frequently produced by the conjoint action of the causes above enumerated—that which may be said to be characteristic of arctic people, though by no means confined to them—is *melancholia*, frequently associated with or marked by *perversion of the religious sentiments*, or by *suicidal propensity*. Dr. Panum represents mental disorder as being very common* in Faroe, affecting 1 per cent. of the population. There is a similarity in the form of insanity among those affected—chiefly melancholy—and frequently of a religious character. He met with many cases of “a quiet form of religious delusion, where the affected individuals saw visions, and believed themselves to be in direct communication with our Saviour and the Holy Ghost, while still more believed themselves possessed of an evil demon, who constantly

¹ Illustrations were given in a paper on “Iceland a New Field for Tourists,” in the ‘Perthshire Advertiser’ for August 2nd and 9th, 1860. For instance, the circumstance is perhaps unique that Reykjavik—a village of only 1200 inhabitants—supports *three* newspapers, of which both the typography and the literary matter are admirable!

² A full and excellent account of the habits of the Icelanders in relation to hygiene or public health is given in the work of Dr. Schleisner, already quoted, or in his ‘Forsög til en Nosographie af Island: skrivet for den Medicinske Doctor-grad.’ Copenhagen, 1849. He was commissioned by the Danish government in 1847 to report upon the diseases peculiar to or prevalent in Iceland.

³ Dr. Sommer, however, makes an assertion to the opposite effect.

forced them to act against their better judgment, causing them to destroy the articles near them, and to strike the by-standers during their fits of fury, and then they generally sank into a melancholic and irritable condition. Both these forms seem often to pass on to fatuity." Dr. Hjaltalin informed me that melancholia is not only very common in Iceland, but the commonest form of insanity in that island.¹ Religious character of the delusions is at least frequent. Women are the most usual patients. Self-starvation in these cases is very rare. Dr. Hjaltalin could remember only one or two cases. He has never used the stomach-pump or other means of artificial feeding in these cases, but has seared the epigastric region with a hot iron. Very few *suicides* occur; when they do, cut-throat is the usual form. The condition of the *liver*² is usually associated with mental disorder in Iceland, at least by the patients themselves. I am informed by Robert Brown, F.R.G.S., that *suicide from melancholia* is not uncommon among the Danish colonists in Greenland, *suspension* being the usual mode. He has favoured me with the following extract from his Diary of a visit to Greenland, dated June 10th, 1867, the locality being Egdeminde, in latitude 68° 42' 39" N.; as well as with the notes appended, contained in a letter of date November 8th, 1869:—"In the morning I was out on the rock taking a series of observations for longitude, which were not very satisfactory on account of the dull unsettled weather; and in the afternoon there was a snow-storm, which gave us a notion of what the winter must be here; indeed, Hr. Bolbroe (the Governor) gave us no very flowery account of the pleasures of an arctic winter in an out-of-the-way 'Annex-Uandelsplad.' Here *he* has his family, his books, his routine of duties, the society of the pastor, the exchange of little dinners, and the moving about among the outposts attached to his district. But with the 'udliggers' or subordinate trader, with little enough comfort, with a native wife uneducated and no companion for him, and no resources of intelligence to fall back on, things go roughly enough. Regrets and melancholy thoughts trouble him, and on the gloomy days sometimes gloomy thoughts are carried into execution, and suicides are not uncommon; indeed, like all crimes, they take runs, and several will follow in succession. Only last winter one of Bolbroe's 'udliggers' committed suicide, and three days afterwards, when Assistant Mürch went, he found him still hanging, and his Eskimo wife making little lament beyond the fact that she would get no more bread, and have to subsist, like the other native women, on

¹ The statistics of insanity and idiocy in Iceland are separately given in Schleisner's 'Island,' p. 32.

² The relative frequency or importance of the principal diseases of Iceland may be illustrated by the fact that, in 1860, Dr. Hjaltalin was anxious for the establishment of an hospital in Reykjavik, of which he proposed to devote one half of its space to leprosy, one third to hepatic echinococci and other diseases of the liver, and the remainder to general acute diseases, including typhus!

seals' flesh and such like." Insanity and suicide among the Danish residents in Greenland Mr. Brown attributed "a good deal to melancholy, want of occupation, and the sad memory of former days contrasted with now." "I heard little," he says "of insanity among the natives. They are a rather stolid race, not given to much excitement of any sort. They have, however, a name for an idiot (a weak-minded man or fool), viz. 'pitlokiok,' and a few cases of insanity are known to have occurred. Some years ago, on the western shores of Davis's Straits, a native was found by the whalers suspended by an 'allunak' (a kind of rope) over a cliff; but whether he had committed suicide or was executed could never be learned. The place has since been named 'Hangman's Cove.'"

Melancholia is also the dominant form of mental disease in Norway. I have elsewhere¹ shown that of 151 admissions into the Christiania asylum at Gaustad, no less than 71 cases represented melancholia, while mania stood only at 43, and dementia at 30. Dr. Campbell, of the Tarban Creek Asylum, near Sydney, New South Wales, told me (in 1862, when I visited the said asylum) that melancholia is also the prevalent form of insanity in that colony. And lastly, as I pointed out in 1859, "Suicidal melancholia is a prevalent type of insanity in Perthshire," especially among the peasantry of its Highlands, who are mostly engaged in the solitary occupation of shepherding amid monotonous and gloomy scenery, exposed to the vicissitudes of a treacherous, and for the most part moist, climate.

Religious melancholia is a form of insanity in regard to which the most erroneous opinions prevail, especially among the clergy. Even where it has been markedly *suicidal*, I have been assured by clergymen engaged in the work of revival in our Highland glens that this form of mental aberration was to be regarded only as the natural and proper fruit of a sense of sin! The patients have been claimed as subjects for *spiritual tendance*, not for *medical treatment*; my very use of such terms as *religious insanity* has been strongly objected to on the ground that religion and insanity *can* have no connection, seeing that it is quite impossible for *religion* to produce other than a healthy and good effect on the human mind! I have been prevented reporting upon insanity as a fruit of religious excitement, or my reports have been suppressed; and I have met with so much clerical and lay bigotry, prejudice, ignorance or error, on the whole subject of insanity either caused by religious excitement, or attended with delusions of a religious character, that I have been struck with the liberal opinions that have been occasionally expressed by a very small minority of the Scottish clergy. Thus A. K. H. B. writes:² "I cannot but regard those religious biographies which we sometimes read, in which worthy people of *little*

¹ 'Insanity and Lunatic Asylums in Norway,' *ol. cit.*, p. 20.

² In a paper "Concerning Solitary Days" (*antea cit.*).

strength of character record particularly from day to day all the shifting moods and fancies of their minds as regards their religious concerns, as calculated to do a great deal of mischief. It is founded upon a quite mistaken notion of the spirit of true Christianity that a human being should be ever watching the play of his mind as one might watch the rise and fall of the barometer, and recording phases of thought and feelings, which, it is easy to see are, in some cases and in some degree at least, *the result of change of temperature, of dyspepsia, of deranged circulation of the blood*,—as though these were the unquestionable effects of *spiritual* influence, either supernal or infernal. Let us try, in the matter of these most solemn of all interests, to look more to great truths and facts, which exist quite independently of the impression they may for the time produce on us, and less to *our own fanciful or morbid frames and feelings*” (p. 219). The late Archbishop Whateley, in a paper on the “Duty and Method of Instructing,”—especially the lower orders (p. 133)—gives some most judicious advice regarding *religious instruction*.¹ Dr. Chalmers, in a sermon on “Christian Conversation” (p. 719)² does the same as regards *e.g.* the proper time and place for preaching. A paper on the “Wrongs of my Boyhood,” in “Cornhill Magazine,”³ gives a graphic and truthful account of the effect of morose religious teaching on the young: while excellent remarks have been made by the Rev. M. B. Hope, M.D., on *Religious Melancholy*, in the “Christian Treasury.”⁴ In the latter case the liberality and justice of his opinions are doubtless attributable to the fact that the author combines in himself the separate, though closely allied, professions of physic and divinity. His and the other utterances above quoted are marked simply by sound common sense, uninfluenced by sectarian or general bigotry.

V.—The Functions of the Tympanum. (A note to an “Original Communication,” thus entitled, in this ‘Review,’ in the year 1867.) By JAMES JAGO, M.D. Oxon., A.B. Cantab.; Physician to the Cornwall General Infirmary.

In the essay alluded to I gave a general sketch of the mechanism and movements of the tympanic membrane and ossicles. In directing, however, attention to certain characteristics of this apparatus, whose influence had, as it seemed to me, been overlooked in investigations into the uses of its several parts, I refrained from entering into particulars which had no obvious bearing on physiological acoustics.

In ruminating lately on the plan of this complicated apparatus I

¹ ‘Good Words’ for March, 1860.

² ‘Sermons,’ vol. ii.

³ January, 1861, p. 95.

⁴ April, 1861, p. 163. On same subject, compare also same Magazine, vol. for 1858, pp. 279 and 316.

have been led to think that it may still involve adjuvant principles in its construction which have hitherto remained undetected, and that a more detailed notice of it may prove elucidatory of the conclusions I had previously recorded. Of course it will be apprehended that in the description I am about to give I keep in view physiological sufficiency rather than anatomical completeness.

The irregular tympanic cavity is very flattened from without inwards—the space to be spanned by the ossicula, malleus, incus, and stapes, in order to connect the membrana tympani, which forms the greater part of the external wall, with the membrane of the fenestra ovalis, which is situated in the internal wall. From the head of the malleus and body of the incus, which are joined together above the level of the top of the membrana tympani, descend in a somewhat tapering manner two nearly vertical limbs. That of the former consists of a neck and a manubrium, which is imbedded in the drum-head from its upper margin to its centre. That of the latter, free from other contact, bends inward at its foot to be articulated with the head of the stapes; and it lies rather posterior to the manubrium, because the articulation between the two outer bones is of such a nature that the head of the malleus is received into a cup in the body of the incus, which looks upwards and forwards. Hence such rotation between these bones as this joint permits is chiefly such as would carry the foot of the manubrium forwards, and that of the long limb of the incus backwards. The crura of the stapes, unlike the two limbs just spoken of, lie in a horizontal plane and project (to converge upon the neck or head) from an oval plate of bone which is adherent to the membrane of the fenestra ovalis, and moves to and fro in the opening as far as a ligamentous connection with it permits. In this way do the ossicles bridge over the space between the said membranous (flexible) parts of the outer and inner walls of the drum.

But the malleus and incus have each another limb; that of the former is a long slender blade (*processus gracilis*), which springs perpendicularly from the front of the neck, and proceeds obliquely downwards and forwards into the Glasserian fissure, to whose sides it is fixed by ligament and bone. That of the latter (*crus breve*) proceeds, in a conical fashion, nearly horizontally backwards to be strongly attached at the extremity by short ligamentous fibres to the posterior wall of the tympanum. In this way do the two larger ossicles, unaided by a third, virtually bridge the tympanic cavity from bone to bone. These limbs may be regarded approximately as having a common axis which passes through the pivot-point of the incudi-malleal articulation.¹ The malleus can only rotate about the axis by twisting its *processus gracilis*, the incus by twisting the said ligamentous fibres.

¹ Helmholtz, 'Die Lehre von den Tonempfindungen,' S. 201, describes these parts as having a common axis.

Furthermore, there are at least two other ligaments which support and limit the movements of the malleus. The round ligament drops vertically from the roof of the drum to the head of the bone. And Toynbee¹ points out that the tubular sheath of the tendon of the internal muscle of the malleus is in reality a ligament of about three fourths of a line long, tying the root of the manubrium (below the proc. grac.) to the cochleariform process, that is, in an inward and slightly forward direction. Could we imagine the fibrous layers in the drumhead and the tubular ligament as interwoven, we might describe that membrane as an elastic funnel, looking mainly outwards, but also downwards and a little forwards, kept in shape further by the inward attachment of its pipe.

In speaking of the tympanic muscles we may confine our attention to the directions of their terminal tendons, because these determine their lines of action.

The tendon of the internal muscle of the malleus, which is by far the most powerful muscle, has already been mentioned. That of the stapedius proceeds from the pyramid directly forwards to be inserted into the neck of the stapes.

J. Müller and others have affirmed that there are but these two muscles in the drum, inasmuch as what had been described by anatomists, in one or more instances, as muscles are only ligaments. But standard anatomical works, nevertheless, continue to insist that at least one of these bundles of fibres contains some that are “distinctly muscular, but concealed by a band of fibrous tissue.”²

Over all these structures is reflected the mucous membrane of the drum, the layers of which between the crura of the stapes are separated from each other only by connective tissue and a few vascular and nervous twigs.

If we now seek to comprehend the movements of which the membrana tympani and ossicula are susceptible, I have already pointed out how we must consider, in conjunction with them, those of the labyrinthian fluids and the membrane of the fenestra rotunda.³

“In this way the middle and inner ears, taken together, are so associated that an inward movement of the membrana tympani, by occasioning (if through a less space) a similar one of the stapes, will cause a flow of the whole of the labyrinthian fluid, so that every portion of the expansions of the nerve receives the watery impulse, and that the membrane of the round window juts outwards. Should now the drumhead move outwards, drawing the ossicles after it, the pressure of the air upon this membranous window would force it inwards, and push the whole body of Cotunnian fluid before it,” and

¹ ‘Diseases of the Ear,’ p. 133.

² Quain’s ‘Elements of Anatomy,’ sixth edition, edited by Sharpey and Ellis, v. iii, p. 51.

³ ‘Rev.,’ v. xxxix, p. 48.

“any movement inwards or outwards originally impressed by the tympanic air upon the membrane of the round window . . . makes the drumhead move outwards or inwards.”

This description confessedly left out of consideration any modifications of the results that might arise from muscular action and changes in the amount of pressure exercised by the tympanic air upon the fenestra cochleæ which might be due to “an inward or outward excursion of the membrana tympani.”

I hope to instance here how, by endeavouring to supply such omissions as those just alluded to, we may get new insight into the scheme of the middle ear.

We may perceive herein, I am persuaded, a reason for the form and adjustments of the stapes :—From its having a flat plate for a base, of which only a very small portion is covered by the crura, it is guided steadily to and fro in the fenestra under variations in the elasticity of the tympanic air; and as the crura abut upon a bone that proceeds at right angles to their plane to meet the bone that moves with the drumhead in a joint, the said variations act immediately and symmetrically upon the remaining portions. Thus, as far as rotation at that joint is easy the base of the stapes (or the membrane of its fenestra) is as sensitive to any variations in pressure of the tympanic air as is the membrane of the round window. Assuming the fenestræ of equal areas, the membranes will be equally affected by such variations. Assuming them synchronously affected, the Cotunnian fluid may not only be stable, but remain unmoved between the balanced forces. If the variations be due to intra-tympanic sonorous waves, they may even be suppressed before reaching the acoustic nerve.

Thus, one of the prominent conclusions in my essay, that the membrane of the round window should repel sonorous waves from the labyrinth, is corroborated by a totally different line of reasoning.

Besides, in such an event as a forcible inflation of the drum through the Eustachian tube, did not the augmented pressure act immediately upon both fenestræ, its impulse at the round window, combined with the strain at the oval one, by the consequent extrusion of the membrana tympani, might endanger the integrity of the membranes of the fenestræ. A like accident might happen from forcible exhaustion of the air in the drum.

Having, as I hope, demonstrated that the labyrinthian fluid is protected from being immediately influenced by changes in the density of the air within the tympanum, I now propose to inquire whether it is obnoxious to being mediately disquieted through their action upon the drumhead. Or rather, remembering that the drumhead is moved in a like manner by a condensation of the air upon its inner, as it is by a rarefaction of that upon its outer surface, and *vice versâ*, I propose to inquire how far any displacements thus impressed upon the drumhead may be conferred upon the ossicles,

and whether any machinery exists to obviate injurious acoustic or mechanical effects that might arise from their excess.

Since the drumhead in its ordinary position of rest projects into the cavity of the tympanum, it can be carried no further inwards than as far as may suffice to take up any slackness it may have, and to exhaust its elasticity. Such movement as may occur will be in a direction opposite to its outward aspect, that is, the foot of the manubrium will strive to withdraw inwards, upwards, and a little backwards, rotating about its upper or marginal end. Rotation about this end will, however, be impeded by the presence of the *processus gracilis*, which must be twisted or bent before such a movement can be effected. As far as it is effected it would tend to throw the head of the malleus a little outwards and forwards, and thus to tilt the foot of the short limb of the incus a little inwards, whilst this limb rotates on and from the malleus a little should its foot be held backwards. Hence the displacements that may happen from an inward excursion of the *membrana tympani* are but very minute as respects itself, and those entailed in consequence upon the ossicles are of such a character as not only to be still more minute, but practically to annul each other by the time they reach the stapes. We may satisfy ourselves, nevertheless, that the drumhead, when it has air of equal elasticity on its two surfaces, is capable of retreating into the tympanum in some degree, by sucking some air from the cavity through the Eustachian tube, when we may distinctly feel it retreat a trifle.

On the other hand, if its connections allow it, the *membrana tympani* might be carried outwards until its middle point is as far outside its margin as it usually lies within it. In making this excursion it would tend to transport the foot of the manubrium, round its upper end, outwards, downwards, and a little forwards; but this cannot be done without putting the tubular and suspensory ligaments upon the stretch, nor without resistance from the *processus gracilis*, which must be both bent and twisted to permit it. There is, however, no ligamentous check against the forward rotation; and in so moving the manubrium might avail itself of the loose part of the drumhead posterior to it, whilst the tubular ligament, by having its posterior point of insertion advanced, going more directly outwards, and by whatever elasticity it possesses, might suffer it to reach a slightly more outward point. Upon the whole, as far as I can see, no other appreciable movement of this bone can result—a movement which we may view as striving to widen the angle between the vertical limbs of the malleus and the incus, by causing the former to rotate about their common axis forwards and slightly outwards. Experiments made upon ears of the living and the dead by blowing air into the drum, or by withdrawing air from the meatus, show that the membrane juts outwards on both sides of the manubrium with but a small effect in compelling this bone to accompany it.

Under the foregoing conditions, why are the ossicles provided with muscles?

As to the stapedius, I perceive that, whilst all anatomists describe it as pulling backwards, some affirm that it also draws the head of the stapes (tilting the bone on its base) inwards, and others that it draws it outwards, and some that it acts in the former manner when the bone is at the bottom of the fenestra, and the latter when at its mouth. I infer from this that its proper office is simply to draw the foot of the vertical limb of the incus backwards when necessary—that is, whenever a forward movement of the manubrium strives to drag the incus after it, a movement which otherwise must be very limited, as the attachment of the short limb of the incus to the tympanic wall forbids it, as well as an opposite movement.

In the same way I look upon the internal muscle of the malleus as acting essentially from without inwards, and as only stimulated to contraction when the manubrium, as above explained, suffers the greatest displacement of which it is susceptible. Its attachment beneath the processus gracilis controls the increase of the angle between the two vertical processes of bone in the transverse direction, and in this way the possible forward rotation of the malleus above described.

The weaker external muscle of the malleus holds the office of moderating any unruly inward strains upon the malleus, which, as these must be of a subordinate kind, we need not trouble ourselves to precisely define; and the ligamentous fibres in conjunction must exert an elastic stay to a like end.

It may be appended that I am not disposed to treat with incredulity the accounts given by some anatomists as to the frequent existence of several other ligamentous threads, or even muscular fibres, passing from the ossicles to the tympanic parietes; for the foregoing survey confirms me in the inference to which I had before been led by experimental observations, that the whole of the ossicular machinery is adapted for keeping the tympanic and labyrinthian membranes in a constantly *lax* state, that is, supported as near as may be on all their surfaces by the barometrical pressure. However, as in the very nature of the contrivances accidents must happen by which the equality of aerial pressure on the two surfaces of the membrana tympani must be momentarily absent, the muscles may exercise a steadying interference to ward off damage when the drumhead is assailed by violent aerial gusts; or, as the Eustachian tube may awhile neglect to duly equalise the internal pressure with the external, by which the manubrium may be retained a little out of its proper site, the muscles may manage to preserve to the stapes its perpendicularity in its fenestra.

This last remark brings me to another subject on which I would say a few words before I close. In spite of the above general principle, I have, in my essay, narrated many experiments which evince

that it is by no means necessary to good hearing that the aerial pressure on the two surfaces of the drumhead should be *precisely* equal. It may differ materially with no appreciable effect on audition; as, for example, when we blow, short of great excess, extra air into the tympana, until we get a sensation that they are overfull, and feel the membranes jut outwards. Such facts justify me in having treated of the drumhead as a simply "flexible" membrane, dependent within upon the elastic support of the tympanic air, and thus in a condition to respond indifferently to sonorous undulations, whether in a state of condensation or rarefaction. Now, it has been assumed by advocates of the theory that audition is effected by pulsations of the stapes upon the labyrinthian fluid, as the ossicles are being carried bodily to and fro by the oscillating drumhead, that because this is supported on both sides by air the manubrium has a greater tremor imparted to it in proportion as the membrane is larger, or, as Helmholtz expresses it, "Hence it comes that the drumhead, in comparison with the membrane of the oval window, has a relatively large surface, upon which the sonorous waves of the auditory meatus act, and that through the ossicles this action is concentrated and conferred upon the little surface of the base of the stirrup-bone."¹ But it is clear to me that this idea is radically at fault, just because the drumhead is balanced by the aerial pressure upon its two surfaces. For though an aerial sonorous wave may be several feet in length, the excursions of the individual particles of air that compose it are very limited, "diminishing in the ratio of the squares of their distance from their centre or source."² Hence, as the excursions have but comparatively small lengths even at the source, at ordinary hearing distances they must be inappreciably minute, much short of the thickness of the tympanic membrane. Hence no point of the surface of that membrane can make an excursion further than the particle of air does that affects it, and every point of it (impediments apart) will move that much, and then in no instance can any one point draw an adjacent point with it; such point accompanies it of necessity, and size of the membrane can have no influence upon the extent of excursion. Surely the advocates of the theory of bodily ossicular vibration must cease to use the above argument in behalf of it. J. Müller, who viewed the aerial vibrations as reaching the labyrinth without such bodily tremor of the ossicles, had more reason for using language such as has been now criticised, though I think not so much as one who prefers the theory that I ventured to advance in this 'Review.' On this I may say, in quitting the subject, that I have recently tried whether it is possible by applying a probe, with all the force (not only by its own

¹ Helmholtz, *op. cit.*, S. 203.

² Weber, 'Wellenlehre,' S. 503; cited by J. Müller, 'Physiology' (Baly), v. ii, p. 1253.

weight) I could bear on such a sensitive surface as that of the drumhead, so to stop the bodily oscillation of the ossicles (presuming they might so oscillate) as to *materially* lessen the hearing power; but with no such result, whilst the other experiments which led me to entertain the views alluded to remain.

According to a novel theory lately propounded by an able writer, the inelastic but flexible membrana tympani is so adjusted, and retained outwardly concave by muscular force, as to be incapable of yielding at all to a wave of condensation impinging upon its outer surface, whilst it is ever ready to be sucked outwards by one of rarefaction impinging thereon, to be instantly pulled in again by muscular action, the ossicles being bodily transported outwards, and restored to their place with it. However, the author seems unaware of the fact that the membrane may be supported on its inner surface by unusual elasticity of the imprisoned air, and yet audition be good. Had he considered the effects of this, he must, I feel assured, have perceived that the observations just alluded to, such as that of partially overcharging the drums with air, are repugnant to his conception; not to say that it is difficult to understand how muscles could efficiently perform the office of incessant response to elastic action.¹

Official Memoranda respecting Scarlatina and Relapsing Fever.

THREE important memoranda have appeared since our last issue, all of them of an authoritative character: two relating to the prevention of scarlet fever, and one relating to the reappearance of relapsing fever in London.

Of the two relating to scarlet fever one has been put forth by the Medical Department of the Privy Council, and is from the pen of Mr. Simon. It dwells upon the contagiousness of the disease, and—pointing out that the property of infectiousness attaches itself more or less to all matters which pass from the body of the patient during his disease and convalescence, to the dead body and to the clothing, bedding, and other things about—insists upon the necessity of adopting proper precautions of private hygiene. It then directs attention briefly to the provisions of the law, as contained in the Sanitary Act, 1866, which prohibits certain actions which tend to the spread of contagious disease, and to the powers given to local authorities for preventing such spread.

¹ 'Phil. Magazine,' Aug., 1869, "On the Structure of the Human Ear, and on the Mode in which it Administers to Perception of Sound," by R. Moon, M.A., &c.

In the 'Journal of Anatomy and Physiology,' May, 1869, is a paper of mine, "The Eustachian Tube: when and how is it Opened?" which should be read in connection with my Essay in this 'Review.'

The other memorandum—that of the Medical Officers of Health—necessarily travels over the same ground, but goes a little further, inasmuch as it points out not only what measures of public hygiene ought to be adopted, and the powers given for this purpose, but shows also how futile all such enactments as those referred to by Mr. Simon are, when administered by such persons as those now entrusted with the duty of carrying them into operation. The Metropolitan Health Officers have not thrust their opinions forward unasked: their memorandum is a reply to a distinct challenge made to them by the Registrar-General. It is a very natural question to put, how it comes to pass that a body of men appointed for the very purpose of checking such a disease have been so utterly powerless in arresting the current epidemic. The secret is now out. It is difficult, at the present time, to over estimate the importance of the statements of these gentlemen, when they show how their hands are tied, not only by statutory deficiencies, but by the culpable negligence of the authorities under whom they serve to carry out the salutary provisions of the Act of Parliament. They know what should be done, and are willing to do it, but are asked to make brick without straw. We commend their memorandum very earnestly to the consideration of the Home Office, of the Sanitary Commission, and of Parliament.

The third memorandum relating to relapsing fever is also issued by Mr. Simon. It is a brief account of the origin and spread of the disease in London, the known cases being enumerated up to the middle of November. It contains an outline of its symptoms, expressing an apprehension of increased spread during the winter, and probably also of an increase of typhus. The precautions recommended are of course those which are suggested by the disposition of the disease to spread among the indigent and those exposed to the unwholesome surroundings of poverty in a great city. Mr. Simon calls on sanitary authorities to enforce the Nuisances Removal and Sanitary Acts, to abate overcrowding, promote good ventilation and cleanliness in the dwellings of the poor, especially in houses sublet to several families. He calls also on Poor-law authorities to give adequate relief to the destitute, and especially to be liberal to the convalescents from the fever. We hope his exhortations may produce a good result. Destitution is prevailing now in London to an alarming extent, and as usual the Poor-law machinery is showing signs of a break down. The president of the Poor-law Board, in despair, has already before the month of November is out, forwarded to the daily papers a Minute virtually admitting as much, and suggesting the establishment of a correspondence and conjoint systematic action between boards of guardians and voluntary relief associations. There can be no question that this would be a good thing if it could be brought about; but we confess to little faith in its practicability.

Instructions for Conducting an Inquiry into Cholera:—

Issued by the Army Sanitary Commission in India.

INQUIRIES into the nature, course, and phenomena generally of epidemics have hitherto, as a rule, failed to satisfy the requirements of scientific medicine. They have either been carried on in too limited a field, or with no predetermined plan calculated to elicit the whole truth; or they have been undertaken by individuals burthened with foregone conclusions or a pet hypothesis, or it may be, by those devoid of the necessary mental and scientific training to make trustworthy observers. Moreover, even when at times the facts observed have been beyond question, much of their value has been sacrificed by their indifferent grouping, by fallacies in statistics and by errors in indicating mutual relations.

Some such impression of failure in previous inquiries seems to have befallen the Army Sanitary Commission in India; for they observe ('Army Medical Report,' vol. ix, 1869, p. 191), "after a laborious analysis of the papers, and a careful consideration of the results, the Commission have arrived at the conclusion that the only lesson of practical importance conveyed by the papers is, that it is most desirable a special inquiry into the whole subject of epidemic cholera in the East should be undertaken."

In view of this proposed inquiry, they have issued a code of instructions to secure completeness, accuracy, and uniformity of observations; the importance of which induces us to make an abstract of it in this 'Review.'

The questions to be determined are:—What is cholera? What are its laws of origin and extension? What is the real nature of the pathological changes which occur in persons suffering from it? What are the best methods of treating the disease, and by what proceedings may the ravages of cholera be mitigated, or its progress be arrested? (Op. cit., p. 193.)

I.—Objects of Inquiry.

It is proposed to arrange the inquiry as follows:—

First.—To obtain statistics of cholera as accurately as possible for the troops, European and native: for prisons and other public establishments; and for the civil population.

Second.—To ensure the careful reporting of facts regarding the movement of cholera and the sanitary state of troops, prisoners and the inhabitants of towns and villages, to the principal public medical officer of each station, or prison, or town. Meteorological observations should be supplied by existing observatories, and by the officers at stations in charge of meteorological instruments.

Third.—Special microscopic, physiological, and chemical inquiries into the nature and cause of cholera to be conducted—

- (a) In localities where cholera is endemic ;
- (b) In localities always exempt from cholera ;

By officers specially set apart and trained for these inquiries.

Fourth.—In co-operation with such specially appointed investigators, medical officers at stations, prisons, &c., should undertake similar independent inquiries to the extent of the means at their disposal.

Fifth.—The special inquiries should be carried out in concert with existing sanitary authorities in India.

II.—*Statistics of Cholera and Diarrhœa.*

All classes of the population must be included in framing statistics. A cholera chart or register is subjoined applicable to registering statistics in the case of troops, and of considerable collections of individuals in a public institution. For the civil population another procedure is called for ; but it is to be remembered that, “in all statistical inquiries regarding cholera and other epidemic diseases the dates and localities of events are of primary importance.”

The dates of evident increase of any class of fevers, or of diarrhœa and dysentery, indicating an epidemic, and the mortality generally, are to be noted ; together with the date of first endemic case ; the daily attacks and deaths ; and, particularly in the instance of cholera, the date of the first attack and of the first death. The population of the affected place, the number and sexes alive at each age ; the daily number of attacks and also of deaths for each sex and each age, are other facts to be entered. The occurrence of cholera in the native population near British troops, should be accurately registered, to admit of comparison with the course of disease among the troops. Cholera maps furnish additional and specially valuable information.

III.—*Meteorology.*

The points of most importance, in comparing the meteorological elements of epidemic and non-epidemic years, are :—Barometric pressure above the average ; unusual droughts or rainfalls in connexion with their effects on water supply, on local malaria, or other cause of local disease ; rapid changes of temperature ; unusually high temperatures combined with rainfalls ; direction and strength of winds ; stillness of atmosphere, amount of ozone and of fogs ; hygrometric and electric states of the atmosphere and unusual atmospheric phenomena.

IV.—*Movement of Cholera.*

When cholera is approaching a district, are there indications of an altered state of health ? Are diarrrhœal or dysenteric affections, or fevers prevalent, or any other premonitions observable. Of the three forms—the sporadic, endemic and the epidemic, are they precisely similar, or in what respects do they differ in their respective

characters? Do premonitory symptoms prevail among unaffected persons in localities where each of these three forms occur? Are there any appreciable phenomena in the disease, in the locality, in the habits of the people, or in the nature of the seasons and atmospheric conditions, which would obviously account for the existence of one form rather than another, or for the passage of one form into another? Are there facts to show why a sporadic case is not followed by other cases; or why an endemic outbreak does not become epidemic? What is the influence of sex, age and temperaments, of diet, of the general state or habit of body, of race, trades and occupations, of the use of spirituous liquors, of temperate habits, and of aggregation of persons absolutely, or within a limited area? The fact of the communicability of cholera directly or indirectly from person to person should be positively ascertained, both by experiment and observation. To determine the question, the movement of cholera must be precisely traced. All the antecedents and surroundings of a solitary indigenous case must be gone into; and so also those present when an endemic outbreak occurs. Prisons afford excellent opportunities for such inquiries.

When cholera takes on an epidemic form, it will become necessary to investigate very carefully the relation of movements of the population to movements of cholera; to discover the exact number of groups of population which have suffered or escaped within the epidemic region, and whether cholera was, or was not present before the arrival of persons from affected districts. In describing the movements of population between affected and unaffected districts the means of communication should be stated, together with the places, dates, and times of arrival.

Precaution must be taken against the disturbing influence of coincidences, and due consideration given to the following known factors:—*a.* The obvious movement of the disease; *b.* The obvious movement of the population; *c.* The fact that cholera appears without apparent or known movement of the population between affected and unaffected districts; *d.* The fact that cholera has appeared in unaffected districts after arrivals from affected localities; sometimes only in persons arriving; sometimes only in residents; sometimes among arrivals first, and residents afterwards; sometimes in residents first, and arrivals afterwards; *e.* The fact that arrivals take place from affected localities in unaffected localities, without any appearance of cholera following on arrival.

It must not be simply assumed that the arrival in unaffected districts of persons from affected districts, was the cause of the appearance of cholera in the former; nor must those instances only be noted in which disease followed on the fact of intercourse, without noting also those in which intercourse was not so followed. In all statistical comparisons on this point, two facts are indispen-

sable :—*a.* An estimate of the number of localities or groups of population which had communication with the affected centre, together with an estimate of their population ; *b.* The number and population of localities in which cholera appeared after communication, together with the dates of both events.

But the fact to be reasonably shown by other evidence altogether is, that the movement of population was the cause of the movement of cholera in those cases in which dates and localities are well known, and where there can be no doubt that the date of the appearance of cholera was subsequent to the date of communication with affected districts.

Epidemics of cholera have generally travelled in a north-western direction in India, owing, it has been assumed, to the movement of the population in this direction ; but the question arises, whether this movement is not accompanied by corresponding movements in other directions, and whether cholera does not follow in the course of such other movements. In other words, if the movement of population has been oscillatory, has cholera moved in one direction only, or has it also been oscillatory with the population. Again, are the lines taken by cholera epidemics those of the maximum fixed population, or are they not ?

Instances where no cholera has appeared in districts in communication with affected districts should be noted ; and also, in the case of districts enjoying an immunity from cholera, it should be ascertained if such immunity has existed notwithstanding communication with affected districts.

The next question to ascertain is, what is the relation of communicability to the phenomena of endemic and epidemic cholera ? Are endemics and epidemics made up solely of cases arising from well ascertained communication of the disease ? Or, is communicability only one of the elements of the movements of cholera ? If so, what is its importance in comparison with that of other elements ? Are there facts to show that the phenomena of epidemics are coincident with general causes such as :—general telluric disturbances ; a generally disturbed or contaminated state of the atmosphere ; general vital disturbances affecting animals as well as man and probably also vegetable life ; and these phenomena existing not only in the epidemic region, but also in distant parts of the earth.

Inquiry should be made to ascertain why endemic or epidemic attacks of cholera come to an end. If atmospheric conditions are the causes, do these undergo alteration when the disease declines and disappears. If defective sanitary conditions are determining causes, why do they cease to act after a time ? If cholera spread from person to person by contagion or cholera poison, how does it happen that the agent or poison communicated ceases to act after a time ?

When cholera prevails it should be examined how far it can be

mitigated by better diet and regimen, and by sanitary measures, and whether the progress of sanitary improvements has been coincident with decline of the disease.

V.—*Sanitary Inquiries in Special Districts and Localities.*

It is advisable to select places for special inquiries:—*a.* Where cholera never breaks out; *b.* Where cholera is endemic; *c.* Where cholera prevails epidemically; and to obtain corresponding comparable data for each locality. Various topographical points will demand inquiry; the height of locality above the sea-level; its height or depression in relation to the surrounding country: its position in reference to the sea, to a river, lake, marsh, &c., and the direction of the prevailing winds relatively to any one of these. Also, whether the level of the locality be above or below water level in the adjacent river or lake: the prevailing configuration of the surface; the abundance or absence of vegetation; the existence of ravines likely to generate malaria, or to conduct it from neighbouring malarial districts; the geological structure, the soils and subsoils; the state of cultivation and of drainage of the land; the nature of the produce raised; the relative position of inhabited buildings, and the condition of sanitary arrangements in and about them. Inquiry should also be made as to whether marsh-fevers are frequent among the population, and what their sources: also whether cases of cholera or diarrhœa are influenced by malaria, and whether cases of marsh-fever have passed into a choleraic state or into cholera.

VI.—*Special Local Inquiry.*

As already stated, this inquiry is intended to be conducted by specially appointed observers in particularly selected districts, and to include a microscopic examination of air, water, soils, articles of food, organs and tissues, vital fluids, including secretions and especially diarrhœal and cholera discharges. What may be called microscopic *constants* for each selected locality *when cholera is absent*, must be kept in view in order that these observations may be compared with another set of such constants obtained *during the prevalence of cholera* in the same localities.

We have not space to extract the minute instructions laid down for carrying out these special inquiries into the microscopic state of the air, soils, and water, and into the food, and discharges of patients, &c., but must refer those who would wish to learn them to the appendix of the Army Medical Report, from which we have made the preceding abstract.

The instructions conclude by suggestions for conducting chemical inquiries. These are pretty obvious and need not be here reproduced.

Chronicle of Medical Science.

CHRONICLE OF MICROLOGY.

By J. F. STREATFEILD, F.R.C.S.,

1. PHYSIOLOGICAL MICROLOGY.

Pacinian Corpuscles.—In the skin of the beak of the duck and goose, says Dr. Grandry, though after the common type of the corpuscles found in other birds, they are, nevertheless, different in the structure of the central bulb, and approach to those described in the woodcock by Leydig.

They are formed of a connective envelope and of a central bulb. The connective envelope is made up of two clearly distinct parts; one, external, of concentric capsules, and the other internal, of connective tissue of fine fibres crossed in all directions, but whose inner parts seem, nevertheless, to form layers concentric to the central bulb, at least after adding acetic acid. The inner part of the envelope is so much coloured as to hinder the examination of the corpuscles, if acetic acid has not been added.

In relation to the structure of the central bulb, two kinds of Pacinian corpuscles are discernible; the first altogether resemble the corpuscles noted in birds, the second present much analogy to those described by Leydig.

In the duck, as in the woodcock, one finds in the central bulb rounded and quadrangular bodies in two rows at the sides of the central fibre (central canal of Leydig), regularly distanced the one from the other, and separated by finely granular matter, having no relation with the envelope, from which they are rather distant, and separated from it by the same substance which separates them the one from the other.

The bodies show in their interior a rather opaque point, which may be considered as a nucleus or nucleolus, according to their interpretation as cells or nuclei.

The central fibre of the Pacinian corpuscles of the duck or goose seems to resemble altogether that of the corpuscles of other birds; but in these animals one finds the terminal extremity of the fibre very voluminous and more granular than that found, *e. g.* in the

Pacinian corpuscles found in the pigeon, in the space between the tibia and fibula. I have not found corpuscles in which the fibres, as in the cat, bifurcated near its end; but I have seen a division of the fibres a little way beyond its entry of the central bulb, and then, in one and the same corpuscle, one saw two central bulbs reunited by one end.

All the characters here indicated are seen very readily by the action of weak acetic acid only, but all becomes much more plain if weak chromic acid is employed in the preparation. The woodcock's corpuscles, besides the peculiar bodies, show striæ: I never could find any of them in those of the duck and goose.

The chloride of gold has also a remarkable action on those corpuscles. If the skin of the beak of the duck or goose in a solution of chloride of gold, of 1 in 100, for twenty-four hours, the action being complete, one sees the whole of the bulb deeply coloured, and continuing with the fibre in such fashion that this seems to end as a swelling, which is no other than the whole bulb. This reaction seems evidence in favour of Leydig's opinion, which regards the whole bulb as the termination of the nerve; but the less complete degree of reaction, and hyperosmic acid especially, show quite clearly the fibre continuing entirely through the central bulb, and that, at least, the peripheral part of this has not the same structure as the centre, and that that which Leydig considers as a canal, is the immediate continuation of the axis cylinder of the fibre which enters the corpuscle. If the chloride of gold has acted incompletely, one sees forthwith that the centre is attacked, then the summit; even the whole is not unfrequently attacked, and the fibre is more marked in the centre.

As to the Leydig corpuscles, they take colour just like the rest of the central bulb; and in the case of complete reaction, it is impossible to trace them.

The chloride of gold, which does not attack the envelope, and, on the contrary, colours the entire central bulb, tends to show that the tissue surrounding the fibre has more of the character of nerve substance than of connective tissue.

The hyperosmic acid best shows the central fibre and the granular terminal swelling, yet it also, but less, colours the rest of the central bulb. By this reagent one sometimes sees the central fibre end as a little swelling in the midst of the ordinary terminal swelling. Beside the Pacinian corpuscles, in the beak of the duck and goose is found another form of nerve-ending, as to the structure of which I am doubtful, especially as to the termination of the nerve. The author then refers to the plate accompanying his paper.—*Journal de l'Anatomie*, &c. (Robin's), July and August, 1869, p. 390.

Termination of the Skin-nerves — Meissner's Corpuscles.—Dr. Grandry found he could obtain the best results in the above investigation, by the reagency of a bichromate of potash, or of Müller's solution. Of Meissner's corpuscles, he says:—The envelope is made up of condensed connective tissue, as Kölliker admits, and, with

him, he believes that the transverse nuclei seen on the surface, especially by action of acetic acid, belong to the connective tissue; in no case could he find the spiral disposition of the fibre reduced to the state of cylinder axis. As to the central bulb, he considers it altogether analogous to those of the Pacinian corpuscles, referring in this to that which he has said of the reaction of chloride of gold on them. But yet he would call attention to the existence of formed elements within this central bulb, a fact sometimes denied.

The course of the medullary fibre is exactly such as most observers describe it—it rolls in large spiral turns around the corpuscle, and thus reaches its summit always within the envelope, without penetrating the central bulb. Does the medullary fibre always take its course external to the central bulb? This the author does not believe, for he has, by use of osmic acid, distinctly seen the fibre altogether within the envelope, and, equally, Müller's solution has shown it him penetrating the central bulb, yet furnished with medulla. As to the way of their ending, opinions vary much. Rouget says that the fibre ends in the central mass, being, in such fashion, continuous with it, that the central bulb might be considered as the fibre expanded, and, as for him, the formed elements are analogous to those found in the terminal plates of the muscles. In the author's experience, the nerve does not end in becoming continuous with the central mass, but indeed with the formed elements formed within it; and this quite reminds one of the terminal enlargements of the Pacinian corpuscles. The author then relates the rest of his observations made on transverse sections of Meissner's corpuscles of the pulp of the fingers, treated with Müller's solution. Within them one sees bodies more or less spherical, granular, varying from $\cdot 008$ to $\cdot 01$ mm., which seem to be often isolated, and to have no relationship with a fibre or with the envelope, and are situated most frequently near to the vertical axis of the corpuscle. Attentive examination of good preparations allow a distinction, beside the spherical bodies, of excessively pale fibres, at different levels, not anastomosing, going off from the envelope, not lost in the granular substance, but continuous with the formed granular bodies, so that the latter seem pediculated. A noteworthy fact is that the fibres leaving the envelope do not go directly to the granular bodies, but, before reaching them, describe greater or less sinuosities. The author has seen the double-contoured nerve-fibre pass into the interior, become reduced to the state of cylinder-axis, and continuous with the granular bodies.

Finally, says the author, the only difference between Meissner's and Pacini's corpuscles is, that in the skin corpuscles the fibre gives off a greater number of endings; though, in the Pacinian, three may be found.—*Ibid.*, p. 395.

The arachnoid space a lymphatic space, and its connexion with the perichoroid space.—Dr. G. Schwalbe, of Amsterdam, has, in the course of last year, described a system of cavities between the choroid and the sclerotic, a perichoroidal space, and expressed the opinion

that this might be a lymphatic space. He says:—I have now succeeded in the production of certain evidence in favour of these suppositions by a single experiment, which at the same time demonstrated another important matter of fact, namely, that the arachnoid space of the brain and spinal marrow is likewise a lymphatic space. The experiment consists in this, that in an animal bled to death from the femoral artery (for the purpose I have generally made use of rabbits, but in dogs also the same result is easily attained) one cautiously lays bare a portion as small as possible of the *dura mater cerebri*, and then beneath it, by a puncture, a solution of Berlin blue with steady force is injected into the arachnoid space.

As a constant result one gets in the first place a fine filling of the lymphatic vessels and glands of the neck. A closer investigation shows that the connection of the arachnoidal space with the lymphatic vessels of the *basis cranii* takes place by the jugular foramen. It shortly comes to pass that the lymphatic twigs, passing out upon the under surface of the basilar part of the occipital bone and the superior cervical vertebra, where they form a plexus provided with small lymphatic glands between the processes of the *longus capitis* and *colli*, out of which lymphatic vessels are evolved which are dispersed in the direction of the lateral parts of the neck, about here to pass away in the deep cervical glands. The injection often reached to the inosculation of the lymphatic vessels in the veins and beyond them, so that in the lower part of the neck one sees the veins which go to form the *vena cava superior* filled with blue injection.

As was to be expected the arachnoidal space also of the spinal marrow, in its whole length, is filled by the above procedure. In one case the lymphatic glands were in particular well filled by a successful injection. The injection had not penetrated to the thoracic duct. I believe that this result admits of no other interpretation than that the arachnoidal space of the spinal marrow stands in direct connection, not only by interposition of the arachnoidal space of the brain, with the lymphatic system.

With the perivascular canals of the brain and spinal marrow described by His, as well as with the lymphatics of the pia, the arachnoidal space stands in no direct relation. The injection matter reaches easily into the subarachnoidal spaces, but always remains separated by the pia-mater from the lymphatic system of the brain. A communication is, in the first place, probably permitted between the efferent lymph courses on or after their passing out by the cranial aperture. None of the injection has been observed to have passed into the ventricles of the brain.

A quite peculiar interest envelopes our injection experiments, in that also lymphatics are filled which have been hitherto but little or not at all studied. One gets in this way a beautiful network of fine lymphatics injected in the nasal mucous membrane. It appears, further, in this, that the space filled with the *perilymph* between the cutaneous and bony labyrinth is a lymphatic space, there the injection of the arachnoid space reached it by way of the *Porus acusticus internus*. Finally, there yet remains to be mentioned, as the most

important result, the injection of the perichoroidal space. The injection fluid reached from the arachnoid space through the optic canal into the orbit, and in the next place filled the space between the inner and outer optic sheath, which subsequently appears as if it were a continuation of the arachnoid space; the injection at the same time enters a space which is found between the retractor bulbi and the optic nerve, thence it passes directly into the capsule of Tenon, from thereabouts to extend into the perichoroidal space by the openings of communication by me lately described. The latter is, by the procedure described, often so completely injected, as one easily obtains this by a puncture through the sclerotic.

We thus see lymphatic currents of the three higher organs of sense in close connection with the arachnoid space. This appears to be a common reservoir of lymph abstracted from the organs of sense above mentioned, and thereby obtains a high significance. The opinion that the serous sacs are no other than lymph spaces, gains new support by our experiments.—*Centralblatt für die Medicinischen Wissenschaften*, June, 1869, pp. 465-7.

Red blood-corpuscles of oviparous vertebrata.—Mr. W. S. Savory, in a paper read before the Royal Society, states that, of all vertebrata, the red corpuscle is, in its natural state, structureless. When living, no distinction of parts can be recognized; and the existence of a nucleus in the red corpuscles of ovipara is due to changes after death, or removal from the vessels. As the oviparous red cell is commonly seen the existence of a "nucleus" is too striking an object to escape any eye, but that it is so is due to the circumstances under which the corpuscle is seen, and the mode in which it is prepared for examination. It is possible to place blood-cells under the microscope for examination so quickly, and with such slight disturbances, that they may be satisfactorily examined before the nuclei have begun to form. They may then be shown to be absolutely structureless throughout; and, moreover, as the examination is continued, the gradual formation of the nuclei can be traced. The "nucleus" first appears as an indistinct shadowy substance, usually, but not always, about the centre of the cell. The outline of it can hardly, for some seconds, be defined; but it gradually grows more distinct. Sometimes it never forms so as to be clearly traced in outline, but remains as an irregular shapeless mass, in its greater portion very obscure. Sometimes only a small part, if any, of an edge can be recognised, most of it appearing to blend indefinitely with the rest of the cell substance. Sometimes it happens that in many corpuscles the formation of a nucleus does not proceed even so far as this. No distinct separation of substance can anywhere be seen, but shadows, more or less deep, here and there indicate that there is greater aggregation of matter at some parts than at others. Occasionally some of the cells present throughout a granular aspect. When the nucleus is well defined, the cell wall is strongly marked; when one is confused the other is usually fainter. This, however, does not apply to colour; on the contrary, when the nucleus is least coloured it contrasts most strongly

with the surrounding cell. As a rule the wall of the cell is more strongly marked than the nucleus.

To the objection that the nuclei are present all the while, but are at first concealed by the surrounding substance, the author replies that, when one cell overlaps another, the lower one is seen through the upper clearly enough to show that the substance of these cells is sufficiently transparent to allow of a nucleus being discerned if it exists. When a nucleus is fully formed it hides that portion of the outline of a cell which lies beneath it.—*Annals and Magazine of Natural History*, July, 1869, page 69.

Red and white Corpuscles in Blood-stains.—Dr. Joseph G. Richardson, of Philadelphia, has attended closely to this matter for the purposes of medical jurisprudence. If the stain, at any time in question, be of blood, is it of man, or of one of the lower animals? For moistening the dried blood clots the author has made use of pure water. He refers to some previous investigations he has made ('*Pennsylvania Hospital Reports*,' 1869), saying, "In the progress of some researches upon the distension of the white blood-cells, when acted on by water, I have often incidentally noticed that many of the red corpuscles become, after a time, so transparent and colourless by the solution and abstraction of their 'hæmato-crystallin' that they are quite invisible under a power of 400 diameters, and appear to be in reality dissolved, as stated by Prof. Wyman, M. Ch. Robin, and other authorities; yet, when closely scrutinised under a $\frac{1}{25}$ immersion objective, their faint transparent outlines can still be detected, thus confirming Prof. Beale's assertion that, 'with the higher powers, not only do we meet with extremely minute corpuscles, but many of them are so very transparent that they could not be seen at all under a low power. Extremely transparent bodies are demonstrated under high powers, which would certainly be passed over by those in ordinary use.'" The author goes on to say that his "results seem to prove a very marked difference in density, if not in constitution, between the external and internal portions of the blood disks." Yet he would not go so far as to affirm of the red corpuscle that it has a proper cell wall.

Besides human blood, that "of an ox, sheep, pig, chicken, turkey and canary bird, most of them dried in a thin film upon a slide, and all dried in a mass upon paper or muslin, were carefully examined, and little difficulty found in distinctly perceiving that the colourless stroma, with its 'straight or slightly waving filament, sometimes more fibrous, sometimes more wrinkled and homogeneous' (Virchow), so long mistaken under lower power for a mass of fibrin, was actually an aggregation of decolourized red corpuscles, with rare filaments of fibrin, and white blood-cells imbedded in it." Dr. Richardson concludes that, "as during the last three or four years opticians have furnished immersion lenses of $\frac{1}{25}$ and $\frac{1}{50}$ of inch focal length, which, with the highest eye-piece, give an amplification of about 2500 and 5000 diameters respectively, thus rendering, with the former, the apparent size of a red disk from fresh human blood, five sevenths

of an inch, while that of a corpuscle from ox blood is but half an inch across, and consequently little more than half the area, as seen upon the stage, it seems as if any careful observer might now, with the aid of such objections, be qualified to pronounce a positive opinion."

The author, by his numerous special investigations, disproves the possibility of any considerable variation in diameter of the corpuscles, of the same blood, occurring by more or less rapidity of desiccation—a plausible objection of Virchow's. He affirms, by calculation given, that "the outer portion of the corpuscles" has no elasticity. Although the red disk, rendered spherical by the action of pure water, is thus altered, he determines that "its parietes undergo no real dilatation in the process; further, the corrugated appearance assumed by the corpuscle when any portion of its internal constituent is removed by exosmosis affords some evidence, that, however much the cavity is decreased, its limiting membrane suffers no actual diminution in superficial area."

"That the blood-corpuscles of a few mammals approach so nearly in size to those of man as to render their distinction doubtful," is of little importance, for justice rarely requires but that a positive diagnosis shall be made between human blood and that of animals which are commonly slaughtered for food, of all of which "a first-rate $\frac{1}{2\frac{1}{5}}$ inch objective will enable us to determine easily and beyond all question."—*American Journal of the Medical Sciences*, July, 1869, pp. 50—58.

Epithelium of the Joints.—The representation made by v. Hueter (Dr. Gerlach writes), concerning the interior lining of the synovial membrane, has produced much controversy, which was almost wholly exercised in the interpretation of the silver representations. Among the opponents in the first rank there stands to it Schweigger-Seidel, who defined the silver representations of the synovial membrane as the fluid tubules of v. Recklinghausen, made manifest by silver for purely artificial productions, beginning in the irregular, and as concerns the form, accidental combination of the silver with the albuminate of the fluid of the neighbouring cells. In regard to the synovial membrane he allows already Böhm the warranted interrogation, how comes it then that by the silvering of a youthful patella the true cartilage cells, notwithstanding all albuminate of the synovia adherent to them, always show their definite familiar forms, whilst in those parts of the same patella described by v. Hueter, not exposed to the friction of the joint, the transition of the cell-forms of the cartilage into those of the horny and epithelioid tissues is evidently demonstrable.

About the contest concerning the signification of these forms and their tendency to give another appearance by the chemical circumstances of the arg. nit., I tried if it were not possible to produce the same appearances by means which had nothing in common with the silver solution in its effect. This I attained fully in the method devised of v. Landois, by sulphate of lead.

If one cuts from any articular surface, *e. g.* from out of those parts of the foot of a calf which are not covered by true cartilage,

but already by the processes of the synovial membrane, what one readily observes by the obviousness of the vessels, and they are placed after careful washing away of the synovia adherent to them, for five to ten minutes in a one per cent. solution of acetate of lead, brings them hereupon after repeated washings in very thin sulphuretted hydrogen water until a slight browning has appeared, one thus gains representations which entirely agree with those obtained by the silver method. A careful washing away of the synovia is, however, in this necessary, because, otherwise, the precipitation of sulphate of lead originating in it hides, or at least materially obscures, the appearance of that which is to be found lying beneath it. Then the appearances seen, through the precipitation of the lead, are so similar to those produced with silver, that they can hardly be distinguished one from the other, whilst they, nevertheless, owe their origin to two quite different procedures, so that I hope to have added in a new direction to the foundations for the real understanding of this structure.

Finally, there may be here inserted one other observation. If one produces in the rabbit by establishing a suppuration in the neighbourhood of the knee joint, or by passing a thread through the joint, an inflammation, one thus observes in the first stage, before it reaches suppuration, a clouding of the synovial membrane. If these are treated with silver almost the same appearances as in the normal synovial covering come to light, but the brown silver network now encloses much clouded protoplasmic globules, which in several layers occupy the places of the otherwise transparent spaces.—*Centralblatt für die Medicinischen Wissenschaften*, 2nd Oct., 1869, pp. 689—90.

The (supposed) Dilator Pupillæ of the Iris of the Rabbit.—Dr. A. Gruenhagen, of Königsberg, contributes a paper in elucidation of this subject, to which is appended an illustration. He says:—The assertion I have made that, in the eye of man and of the mammals, no dilator of the pupil exists, I have recently subjected to a repeated re-examination. The results of the very full methods of inquiry of which I have made use have only contributed thereto in corroboration of my view. I limited this time the investigation to the rabbit's iris completely, because these animals, according to the common voice of anatomists, are said to possess a particularly distinct, and, moreover, an altogether peculiarly developed dilator; and, besides that, it had also been by choice made use of in experimenting on the motion of the iris. The methods by which I laid out the fresh iris for microscopic consideration were (1) the carmine staining of Schweigger-Seidel, (2) the chloride of palladium staining of F. E. Schulze, (3) the gilding of Cohnheim. By the first method of treatment the nuclei of the connective tissue elements were especially brought into view in their characteristic form with saturation of red colouring matter, whilst at the same time the contours of the more delicate cells continue to be distinctly maintained. The cell contents, the protoplasmic appears on the other hand in thick layers, only pale rose colour, the intercellular substance altogether colour-

less. As concerning the other two methods, as they have principally to do with the cell-contents and the protoplasma-like matter, and always colour the latter, the former only when it is rich in albumen, bright yellow by use of the chloride of palladium, purple violet by use of the chloride of gold. Cell nuclei and nucleus corpuscles continue at the same time to be well preserved, and frequently even gain in sharpness of outline. According to this, therefore, all the three above-named methods must appear to be adapted to the investigation of certain organs, or parts of organs, preferentially towards the distinguishing, the distribution, and arrangement of the smooth muscular fibres, since the elements of this kind of tissue are not only rich in albumen, but also contain well developed nuclei. One only need take some precaution in their use so far as to observe that not every tissue concerned of the characteristic colouring, and not every spindle-shaped nucleus is regarded as indubitable proof of the presence of smooth muscle. Then also other cellular formations, even elastic fibres, assume, by treatment with the chloride of palladium or chloride of gold, the characteristic colouring, and other elements also of the animal's body contain spindle-shaped nuclei. In every case there were first carried out experiments of isolation that have well succeeded, in the familiar way, quite sufficient by proper application, the physiological function, the place of the appearance and the relation of the structures in question to the neighbouring relations, the determination finally.

In what follows I would limit myself only to proving the results, which I gained by means of the chloride of palladium method, once therefore, because this method, and certainly with reason, is more particularly adapted to the purpose of research after smooth muscle; and again, because in actual practice the other two methods furnish exactly corresponding appearances. I obtained the specimens in the state of transparency to be desired, when I left the uninjured fresh irides, of white or of coloured rabbits, to harden for from twenty-four hours to eight days in 100 Cc. of distilled water, to which ten drops of acetic acid and one drop of a saturated solution of chloride of palladium have been added. Although the solution was only tinged a pale yellow, it, within the time declared, coloured the pigmentless iris equally of a bright straw yellow, and so well hardened the tissue that sections were easily to be made with a razor, when one extended the iris on a cork support, or had imbedded it in glycerine glue. The pigmented irides were only fitted to the attainment of cross sections, the pigmentless, on the other hand, yielded also microscopic objects, either in small segments or also of the whole without further preparation, especially in reference to its posterior surface. Transverse sections and surface preparations, in glycerine, both were put on the object-glass.

The latter, which at least may be placed here, in order to begin with them, allow us to come to the following conclusions:—The posterior surface of the iris was covered by a system of folds, which seemed to extend in a radiating direction from the ciliary to the

pupillary border, partly a direct continuation of the ciliary processes.

With exception of the basis of the ciliary processes, which in the rabbit constitutes an integral part of the iris, there may be distinguished on the whole surface a double nucleus layer, of which the uppermost (posterior) have a roundish appearance, a discoid or money-like shape, and were generally furnished with one only, but now and then also with more (two to three) nucleoli. The contours of the appurtenant epithelial cells were easily discernible, they answered to four or five angles, somewhat distorted and rounded.

Beneath these were lying nuclei in greater number of anomalous form. They were elliptic, their greater axis radially arranged, in the greater number provided with no very distinct nucleoli; in length they measured .008 mm., in breadth .004 mm., and take a very fine fibrous, light-yellow coloured, layer, without doubt, the number of the nuclei corresponding to the cell contours could not further be discovered. The light yellow layer was abundantly broken through by streaks, radially directed, of a deep yellow, which had a course from the ciliary border almost close to the pupillary edge, and here they seem to unite with the circular fibres of the sphincter pupillæ in an arcade-like manner. The folded prolongations of the ciliary processes are so far in connection with them, as in all cases, in those streaks to have passed over the most sloping parts of them. The latter were, as, in carmine preparations, one may be best persuaded, abundantly provided with nuclei, which, according to the form and size of the next, looked like the above described nucleation.

Manifestly these yellow, radially directed streaks represented no other than the arcades regarded as a dilator pupilla. It now is to be questioned whether they truly are made up of smooth muscular fibres.

The author pursues this subject at some length, and in a manner not capable of condensation; he concludes that the acceptance of dilator-arcades (Kölliker and party) is also in the rabbit inadmissible, and that no foundation appears for regarding, as does Henle, in the character of muscle, the posterior limiting layers of the iris, which were first made out by v. Bruch and H. Müller, connected continuously with the basement lamella of the choroid.—*Zeitschrift für Rationelle Medicin*, 3rd Series, xxxvi, 40.

II.—PATHOLOGICAL MICROLOGY.

Granular degeneration of the nerve-cells in insanity.—Dr. Howden, of the Montrose Royal Lunatic Asylum, would in any case of death in a lunatic asylum, have the brain minutely examined, and primarily, *as fresh as possible*. As general results he says—"I have met with an apparent fatty degeneration of the grey substance of the cerebrum in some cases of general paralysis, and in cases of acute mania without paralytic symptoms, but believe it to be a rare condition.

"Second. That in all cases of long-standing insanity, the cells of the grey matter of the cerebrum present a granular appearance;

that this appearance is most intense when the mental excitement has been most severe and long continued, as in general paralysis, epileptic mania, and remittent mania; that it is always accompanied by a deposition of granules of hæmatoisin outside the walls of the capillaries and smaller vessels of the grey substance, and usually by deposits of free granules scattered through the grey substance.

“Third. That similar changes in or around the cells are found in all parts of the brain and spinal cord, with the exception of the cerebellum, where the cells are always (?) free of granules.

“Fourth. That the granules are not generally of a fatty nature.

“A careful examination with a variety of magnifying powers inclines me to think that in many instances, the granules are deposited, not *inside* the cells, but *around* them, as the hæmatoisin is around the blood-vessels; and I have sometimes noticed them scattered along the fibre coming from the multipolar cells. The granules are unaffected alike by sulphuric ether and strong alkalies. In extreme cases the cell becomes converted into an opaque, yellow, horn-like body, in which no trace of a nucleus can be detected.—*Lancet*, July 31, 1869, p. 157.

On the tissue-changes in the inflamed liver.—A. v. Hüttenbrenner, in opposition to the assertion of Joseph, that a granular degeneration of the liver cells begins about a needle thrust into the parenchyma of the liver, writes in confirmation of the earlier statements of Holm. He also found in the neighbourhood of the pin a concentric disposition of spindle-shaped elements, and indeed even after twelve hours altogether in such manner as after one or more days. The proof of this state in the extirpated liver, which after the introduction of a needle, at once had been placed in chromic acid, justifies the supposition that those spindle-like forms in layers are liver-cells, and shows at the same time that the change described is essentially brought about by mechanical subdivision. Accordingly one misses also the spindle-shaped layering around the needle, in cases in which in the penetration accidentally the connective tissue has been met with.

In acute suppuration which has been brought about by irritation of the surface of the liver section by means of ammonia, one finds in the neighbourhood, as also in the interior of the individual lobules, massive cells appearing like pus corpuscles placed around the vessels as in a circle. Then in the inflamed liver, both between the liver cells and within them, even indeed in their nuclei, one finds vermilion, so the amœboid cells containing vermilion and the vermilion test generally will prove nothing here, and one may thence conclude it to have originated only in the local arrangement.

In conclusion the author yet briefly describes the relations of the liver-cells and of the fibrous tissue in abscesses of the liver, cirrhotic thickenings and syphilitic nodes. In all these one perceives in the ground layer by the side of each of the liver-cells in succession at least simple fibrous arrangements, intermediate stages which point out a transformation from the former to the latter.—*Centralblatt für die Medicinischen Wissenschaften*, September 4, 1869, page 631.

CHRONICLE OF PHYSIOLOGY.

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PART I.—ABSORPTION.

AUG. WALLER, M.D., F.R.S.: *Some Observations respecting the influence of Chloroform, &c. in promoting Cutaneous Absorption.* (In 'Practitioner,' Nov., 1869.)

IN this important communication Dr. A. Waller states that his tests to prove that absorption had taken place were physiological, being principally obtained by observing the effect on the pupils, produced by the absorption of certain medicines, such as atropine, aconite, &c.

He observes that it may be shown that the rapidity of absorption is to a certain extent connected with activity of the circulation, as is shown by the non-absorption of medicines from the intestinal canal during the later stages of Asiatic cholera, and also by the interesting experiment of etherising a frog, when it will be found that, whilst before the action of ether, cyanide of potassium is readily absorbed by the skin, and is discoverable in the tongue or any part of the mucous membrane of the mouth, when the animal is subjected to the action of ether, and placed in the same condition, absorption is not found to occur; not so much from the absence of local absorption in the skin, as to the absence of vascular action, for the cyanide can still be detected on the inner surface of the skin exposed to its action, but from the deficient means of transport in the vessels around has remained localized and inert in the system. The chief obstacles to absorption in the human skin are the dry, condensed varnish of the cuticle, and the oily secretion of the sebaceous follicles, and these two causes are so efficient in preventing absorption that many physiologists still deny the power of the skin to introduce into the system any medicinal agent dissolved in water. If, however, continues Dr. Waller, "instead of employing aqueous solutions we place various alkaloids dissolved in chloroform in contact with the skin, we quickly obtain evident symptoms of the absorption of these agents which may be carried to an extent sufficient to destroy life. When a mixture of equal parts of chloroform and tincture of aconite is maintained in contact with the human skin it rapidly produces irritation and vascularization, which, after augmenting during the first two or three minutes, subsides gradually, and the part becomes pale and more or less insensible, with a local decrease of temperature; still later the part becomes insensible to the prick of a needle, though it still retains an imperfect sensibility when pressed upon. In fact, the part is in a state of superficial paralysis, which does not extend to the deeper lesions. This state of anæsthesia will continue for several hours." Comparative trials showed that the anæsthetic effect was essentially

due to the aconite. Besides these local effects of the absorption of aconite, there frequently exist others resulting from its general influence on the system, namely, slight nausea and a sense of depression. Experiments on Albino rats, which consisted in maintaining one of the feet of the animal in a solution of atropine, and examining the pupil from time to time, showed clearly that little or no effect was produced even after half an hour's immersion, when the menstruum was alcohol or water; but with a solution of atropine in chloroform of one per cent. of the alkaloid, dilatation of the pupil was generally obtained in from two to five minutes. Similar results were obtained with a solution of equal parts of tincture of opium and chloroform. It is curious to notice, however, that the addition of alcohol to chloroform does not retard, but rather accelerates, absorption. The addition of astringents to a solution of atropine and chloroform has no influence in retarding the absorption of the atropine. It might, and indeed has been suggested, that chloroform acts by dissolving the sebaceous matter, and in consequence quickly penetrates through the skin, carrying with it the substance in solution. It is, however, impossible to accept this explanation, as alcohol likewise dissolves the sebaceous matter, yet alcoholic solutions remain unabsorbed after prolonged contact with the skin. Experiments were undertaken by Dr. Waller to examine the separate influence of chloroform and alcohol upon the skin, after death, to clear up this point, and these experiments showed that chloroform easily traverses dead skin by diffusion; that alcohol does not traverse the skin, but produces an endosmotic current with water; that skin exposed to chloroform in a state of liquid, or vapour, absorbs a considerable quantity of it; that on traversing the septal skin of the endosmometer, chloroform carries with it a certain amount of any alkaloid dissolved in it; phenomena which sufficiently explain the rapidity of cutaneous absorption during life, without our having recourse to any problematic influence of sebaceous matter on the surface of the skin.

SECRETION.

1. Dr. GIOVANNI SAVIOTTI: *Researches on the intimate Structure of the Pancreas*. (In Schultze's Archiv f. Mikroskop. Anatomie Band v, p. 404. 1869.)
2. U. O. BERNSTEIN: *On the Physiology of the Pancreatic Secretion*. (In the Sächs Akad. Sitzungsberichte, Math. Phys. Class, 1869, and Centralblatt, Oct. 9.)
3. Dr. E. KEMMERICH: *Essay on the Physiological Chemistry of the Milk*. (In Pflüger's Archiv f. d. gesammte Physiologie, Band ii, Heft. viii, p. 401. 1869.)
4. CH. LE GROS and ONIMUS: *On the Elimination of Urea under the influence of Electric Currents*. (Paper presented by M. Longet to the Academy of Sciences in Revue Medicale, An. i, T. ii, p. 432.)

1. Dr. Saviotti's observations were chiefly made upon the pancreas of the young rabbit, which is of loose texture, and otherwise well

adapted for microscopic examination. A few injections were made of the pancreas of the rat, which were successful. Those made on the frog and dog were not satisfactory. The injection used was coloured with Prussian blue, but as alkaline fluids, and, consequently, the pancreatic juice, to a certain extent discharge the colour of Prussian blue, M. Saviotti first made a complete injection of the gland under a pressure of from 15 to 30 mm. of mercury, then macerated it in absolute alcohol, to which a few drops of acetic had been added, and finally, after cutting the gland into fragments, placed these in a mixture of equal parts of distilled water and acetic acid, which not only restored the colour to the Prussian blue, but also rendered the separation of the lobules of the gland more easy. With low powers the gland appears to be made of lobules, through the middle of which a trunk of the duct passes, giving off at frequent intervals branches nearly at right angles. This mode of ramification differs from that observed in the salivary glands, which is more tree-like. The mode of origin of the ducts of the pancreas is of considerable interest; up to a recent period it was supposed that the cells of a lobule form a vesicle, the cavity of which is the commencement of the duct. Langerhaus, however, found in his injections that processes from the central canal penetrate between the gland-cells and reach to the membrana propria of the gland vesicle, processes which he named "pyriform," on account of the form they assume when fully injected. These processes were easily discovered by M. Saviotti, but he found that most of them were continuous with canals which run along the borders of the cells in immediate contact with the membrana propria, and unite the adjoining pyriform or radiary (as M. Saviotti prefers to call them) canals by loop-like connecting branches. And now, since the radiary canals are united by several such looped vessels at different points with the neighbouring canals, a fine vascular plexus results, in each of the meshes of which one gland-cell is contained; each gland-cell, therefore, stands in relation to the origin of one duct, not with one surface only, but aids by several, if only small portions of its surface, in the formation of several ducts, an arrangement which is essentially similar to that recently described as existing in the liver. Even in uninjected specimens the same features may be dimly discerned. In some of the lobules of the pancreas the looped vessels running on the outer borders of the cells could not be discerned, but only the radiating pyriform canals, and in these parts M. Saviotti thinks that there may be a mode of termination by free extremities, distinct from that by plexuses as above described. In some lobules there appears to be an external vascular flexus, but no central canal belonging to it. M. Saviotti thinks with Langerhaus, that whilst a certain kind of cell lines the gland vesicles, there is another form called the "centro-acineral," which represents the transitional form from the cells of the gland-vesicles to those lining the ducts.

The gland-cells are polygonal, with a large round granular nucleus. The contents are also granular. The centro-acineral cells are fusiform, and give off two processes, or sometimes even three, one of

which penetrates between and appears to form attachments to one, two, or three gland-cells, whilst the other is in connection with a similar fusiform cell. These fusiform cells possess large oval homogeneous nuclei, and have clear protoplasm. Further investigation satisfied him that the centro-acineral cells are nothing but the commencement of the larger ducts, and are directly continuous with the gland-cells. In the larger ducts the cells become more and more cylindrical.

2. Bernstein examined the secretion of the pancreas as it was discharged from persistent fistulæ of the duct. The fluid possessed all the digestive attributes of the pancreas, and M. Bernstein holds that the opinion of Bernard that only the tenacious secretion flowing from fresh fistulæ presents the properties of the healthy juice is erroneous. Instead of a canula only the ends of a looped lead wire were introduced into the duct. It has long been known that the secretion is induced by or increased during the act of digestion. M. Bernstein found that during fasting no secretion occurred, but that the flow was abundant during the first hour after ingestion of food, attained its maximum in the course of the second or third hour, then underwent an increase, rose to some extent during the fifth, sixth, and seventh hours, and gradually fell to the fifteenth hour, when it altogether ceased. The two periods of increase are obviously the result of reflectorial nerve excitation coinciding with the entrance of food into the stomach and intestine. Smaller variations of irregular amount were observed, and as was noticed by Weinmann and Bernard, complete suppression occurred during vomiting. This effect is not due to any mechanical influence, since it begins with the mere disposition to vomit. Irritation of the central extremity of the divided pneumogastric suppressed the secretion for a long time. Irritation of the peripheric extremity was without effect, as was also simple division of the vagus. The secretion of the pancreas thus appears to be subject to two opposite nervous influences, both of which proceed from the stomach. In subsequent experiments, the nerves distributed to the pancreas accompanying the arteries were all divided, the effect produced was a persistent continuous flow of healthy juice, the gland was rosy and sometimes œdematous. Poisoning with woorara caused a great increase in the quantity of the secretion. The dry residue of the juice obtained varied from 1.68 to 5.39 per cent., the amount varying with the rapidity of the discharge.

3. Dr. Kemmerich observes that various observations have been recorded showing that casein or the albuminate of soda corresponding to the casein of milk may be obtained by chemical means from pure albumen. Thus Lehmann has shown that casein results from the action of pure alkalis on ordinary albumen; and Dr. Kemmerich himself formerly made some investigations in regard to the chemistry of the milk in which he found that if recently drawn milk were divided into two portions, one of which was immediately analysed, whilst the other was set aside at a temperature of 37° to

40° C., for some hours, the latter constantly contained more casein than the former. In the present communication, he repeats and confirms his former experiments, and observes that the formation of the casein cannot be regarded as a process of coagulation; moreover, that a short exposure to 40° C. is not sufficient to effect the conversion, but that several hours are required. At the same time he finds a diminution of temperature of from 10° to 15° C. below the temperature of the body to render the process much less energetic. The last secreted milk shows the process best, whilst that which has long been retained in the gland shows it to the least extent. His experiment shows further that the process is not continuous but stays at a certain point. Other experiments made for the purpose of determining the formation of fat in milk and in cheese on exposure to the air, seemed to show that the occurrence of fat is to some extent connected with the presence of fungous spores in the milk, and that it proceeds from the splitting of albuminous compounds.

4. The variations which occur in the elimination of urea are an evidence of the greater or less activity of the oxidation of the nitrogenous substances, and, consequently, of the activity of the nutritive processes. The authors thought that the influence of electricity on nutrition might be determined by estimating its effects in causing variations in the amount of urea discharged. They experimented chiefly upon rabbits in consequence of the facility with which these animals can be catheterised, and occasionally they made themselves the subjects of experiment. As a general rule, the electrization lasted half an hour, one of the rheophores being placed upon the hind foot and the other in the lumbar region. The method of Lecomte was used for estimating the urea, consisting in estimating the nitrogen of the urea, treated with the alkaline hypochlorites (thirty-seven cubic centimetres of nitrogen corresponding to one decigramme of urea). The general results obtained were, that the passage of interrupted currents diminish the quantity of nitrogen; that continuous centrifugal currents constantly diminish the amount of urea and augment that of the urine; that continuous centripetal currents augment the production of urea without notably increasing the secretion of urine, which in some instances was even diminished. Further, they are disposed to believe that interrupted currents weaken the phenomena of general nutrition, and that continuous currents by facilitating endosmose and dialysis, increase the activity of the changes occurring in the tissues, and, in addition, that the centripetal current when acting on the central nervous system, causes stronger reaction, a kind of artificial febrile state which explains its effects.

HEART.—RESPIRATION.—ANIMAL HEAT.

1. A. BAYER: *Clinical Evidence that the first Sound of the Heart is due to Muscular Contraction.* (Archiv f. Heilkunde, Band x, p. 270.)
- P. GUTTMANN: *On the cause of the first Sound of the Heart.* (Virchow's Archiv, Band xlvi, p. 223.)

- P. NIEMEYER: *On the cause of the first Sound of the Heart.* (Deutsche Klinik, 1869, Nos. 15 and 16. Also Centralblatt, 1869, No. 23.)
2. SCHEREMETJEWSKI: *On the changes in the Chemistry of Respiratory Process effected by the addition of Combustible Molecules to the Circulatory Blood.* (Centralblatt, 1869, p. 691; and Sächs Akad. Sitzungsber. 1869, p. 154—194.)
3. SIEGMUND FLEISCHER: *On the Influence of Hydrocyanic Acid and Woorara on the Temperature of Mammals.* (Pflüger's Archiv, Band ii, p. 432.)

1. The mode of origin of the first sound of the heart, and whether it originates in the vibration occasioned by the sudden tension of the auriculo-ventricular valves, or is a muscular sound caused by the contraction of the thick muscular walls of the ventricle is a question that has once more been raised. M. Bayer adduces a case in support of his opinion that the first sound of the heart is muscular, in which during life a systolic sound was heard, whilst a post-mortem examination revealed insufficiency of the mitral and bicuspid valves, together with stenosis of the venous orifices. In this case, he is of opinion that the valves were physically incompetent to produce the sound. M. Fräntzel in commenting upon this case, however, observes that the proof here adduced is not very satisfactory, as every one has met with cases of incompetence of the arterial valves, yet in which a diastolic sound was audible. The question, in fact, resolves itself into one of degree. 2. M. Guttman is opposed to the idea of the first sound being a muscular sound, on the ground that in dogs from which the blood had been drained, the sound was very different from that of healthy dogs, and his comparative experiments have led him to the conviction that the first sound is essentially valvular, the muscular contraction only slightly participating in its production. It must be admitted that, however freely dogs are bled, sufficient blood must still remain to render the valves tense, whilst, at the same time, by the papillary muscles contracting, the valves are rendered tense. 3. M. Niemeyer, without adducing any fresh evidence in favour of his opinion, simply holds that the systolic sound has a muscular and not a valvular origin.

4. M. Scheremetjewski made his experiments in Ludwig's laboratory, and employed the apparatus constructed by Sanders-Ezn, and described in the 'Centralblatt,' for 1867, with some slight alteration. He prefixes to his remarks the results of his experiments on the exchange of nitrogen gas in respiration, and in reference to this point states that in eighty-two observations the amount of nitrogen only remained unaltered in seven. Yet in the greater number of the experiments, the difference was so small that it might be attributable to error in the estimates, and in some of them there was slight increase, in others slight decrease of the amount. In twenty cases, on the other hand, the amount of nitrogen was so materially diminished, and in eight so much increased, that it could not be fairly conceived to be due to gasometric errors. And though other sources of error were

possibly present, it was more probable that the alteration was effected during the respiratory acts. The frequency of the respiratory movements in animals were observed to vary to the greatest degree when the animals were first placed in the apparatus, but this was found both by M. Scheremetjewski and by Sanders-Ezn to exercise little influence on the exchange of gases. M. Scheremetjewski now compared the differences in the chemistry of respiration before and after the injection of oxidizable matters into the blood, and the first substance injected was lactate of soda, of which quantities containing from 0.3 to 0.8 grammes of lactic acid dissolved in from 2 to 2.5 centigrammes of water was first thrown in. The effects observed were that both the absorption of oxygen and the elimination of carbonic acid were increased to a considerable extent except in two cases in which the excretion of CO_2 was diminished, though in one of these, after the lapse of some time, an increase occurred, and in the other the decrease was very small. The mean of all the experiments showed that both the absorption of O and the excretion of CO_2 had augmented 1.25 fold, the two exactly counterbalancing each other. The increase of the gas-exchange was proportionate to the amount of the lactates injected, and was maintained for an hour, but never rose to so great an extent that it could not be explained by the combination of the lactic acid injected. That this really occurred seemed to be shown by the rapidity with which an exaltation of the products of its combustion made their appearance, by their quantity bearing a direct proportion to the amount of the lactate injected, and by experiments showing that even when very large quantities were introduced, they rarely appeared in the urine. Now the increase in the quantity of CO_2 exhaled might be dependent on some other augmentation of the respiratory process occasioned by the substance injected. In order to exclude this possibility, M. Scheremetjewski compared the gas-exchange of pure blood with blood containing lactates during its passage through the kidneys in dogs. Here also an increase was observable, whilst simple addition of the lactates to the blood caused no notable alteration. Scarcely any other explanation, therefore, is admissible than that the phenomena observable are dependent on combustion of the lactates. Grape sugar was found to behave itself very differently from the lactates, occasioning no alteration in the gas-exchange, which stands in opposition to the very generally received opinion that sugar undergoes rapid combustion in the body. An increase of gas-exchange analogous to that occurring with the lactate of soda, was observed with carbonate of soda and glycerine, but no remarkable alteration took place with formiate, acetate, or benzoate of soda.

5. Fleischer states that from his experiments hydrocyanic acid cannot be regarded, as Hoppe-Seyler maintains it to be, an antiphlogistic remedy, since a decided diminution of the body temperature only occurs when such an amount has been subcutaneously injected as to produce collapse. With smaller amounts it either remained unaltered or slightly increased. In regard to woorara, he found that

this subcutaneously injected caused a very transitory elevation of temperature, both in fatal and in non-fatal doses. In nearly fatal cases this elevation was followed by marked diminution of temperature, which was persistent.

NERVE. MUSCLE.

1. G. SVIERCZEWSKI and Prof. W. TOMSA: *On the Physiology of the Nucleus and Nucleolus of the Nerve Cells of the Sympathetic.* (Centralblatt, No. 41, 1869.)
2. J. GERLACH: *On the Decussation of the Centric Extremities of the Hypoglossal Nerves.* (Henle and Meissner's Zeits., 1869, p. 1.)
3. *On the Nerves of the Peritoneum.* By E. CYON, Ludwig's Arbelin aus der Physiolog. Arstatt zu Leipzig, 1869.

1. THE following observations on the physiology of the ganglion-cells of the sympathetic nerve have been made in the physiological laboratory at Kiew:—

a. In regard to the movements of the nucleolus. If isolated nerve-cells from the sympathetic ganglia of the frog are examined in the serum of the blood of the same animal in lymph, or in the aqueous humour, and the nucleolus be carefully examined with an ocular No. 3 and No. 8 Hartnack's objective (immersion lens), we may easily convince ourselves in a great number of cases that it changes its position in the interior of the nucleus, and this is especially visible when two nucleoli are present. The movement is usually, however, so slow that it requires great and prolonged attention to observe it; on the other hand, it is sometimes so active that it is impossible to sketch it on paper. Rapid movements occur in hungry frogs more frequently in winter than in summer. Admixture of water with serum accelerates the movements. The movement resembles ordinary molecular movement, being dancing or vibratile, and limited to the extent of a quadrant of the cell. The duration of the movements is various, in many instances it ceases within a quarter of an hour after the isolation of the nerve cell; whilst in other cases, providing evaporation be prevented, it will last twenty-four hours. The dependency of the movements of the nucleolus may generally be referred to chemical changes taking place in the nucleolus and the nucleus. Occasionally no movements can be observed.

b. Action of gases. The nucleus of nerve-cells, when these have been rapidly isolated and examined in fresh serum of frogs' blood appears as a clear vesicle, which includes one, two, or more, rarely three, nucleoli, and usually a few dark moving points. In a few cases the nucleus is filled with an extremely fine granular precipitate. The nucleoli, when thus examined, have a polyhedral form, and a dull aspect. Very careful observation often shows the presence of one or several dark granules in the nucleoli.

Exposure to the action of oxygen or hydrogen in Stricker's gas chamber causes the following changes:—The nucleus becomes clearer,

two kinds of matter can be differentiated in the interior of the nucleoli, dark granules and a clear mass through which these granules are scattered. The number of granules in the nucleoli varies from two to six, but if the nucleus contains two nucleoli each of them may only contain one granule. The author is convinced that the clear substance of the nucleolus exhibits changes of form under the influence of oxygen, becoming contracted, though he acknowledges that such alterations of form, on account of the minuteness of the object, are most difficult to observe. If carbonic acid be made to act on the cell different phenomena are observed—an insignificant quantity of a punctiform precipitate occurs in the nucleus, causing it to assume a darker appearance. The nucleoli become darker, especially at the periphery, and their contour changes from a polyhedral form to a more rounded one. The granules within the nucleolus become gradually invisible, causing its periphery to present a finely granular appearance. The movements of the nucleolus are usually increased.

c. Action of distilled water. This causes enlargement and increase of transparency in the nucleus. The nucleolus exhibits its composition very clearly, that, namely, of scattered dark granules in a clear substance. But, whilst the clear substance rolls itself up into balls, the contour of the nucleolus differs considerably from that which it originally possessed, and the dark granules retreat towards its periphery. The movements of the nucleolus sometimes increase, sometimes remain as before. If CO_2 be transmitted through the fluid an abundant granular precipitate occurs in the nucleus, which is not again dissolved by O or H.

d. Phenomena accompanying desiccation. If oxygen or hydrogen gases, or air, be allowed to play over the preparation the contour of the clear substance of the nucleolus becomes more and more indistinct, and at length vanishes entirely, only a few dark granules remain, which are distinguished from any others present in the nucleus by their near approximation to one another. The addition of water causes the gradual reappearance of the clear substance of the nucleolus. If a current of CO_2 be conducted through the gas chamber at a certain stage of inspiration, the nucleus becomes irregular in form with dentated edges, and the nucleolus entirely disappears. . . . On the addition of water the nucleus resumes its original form and the nucleolus reappears, whilst a granular precipitate occurs in the nucleus.

e. Formation of nucleoli in the nucleus. When the nerve-cells are very rapidly isolated the contents of the nucleus are often quite transparent without granules, or with only a few performing more or less active movements. In a short time however a development of moving granules occurs which gradually increase in size, but diminish correspondingly in number.

In the first instance each granule enlarges independently, but subsequently the enlargement appears to result from fusion, and these processes may be accelerated by the addition of water or serum. In one instance in which a nerve-cell was contained in a mixture of one part of serum, one part of lymph, and two parts of water, the

author observed the formation of several granules in the nucleus, which united to form the nucleolus.

2. After reviewing the statements of previous writers, as those of Kölliker, Lenhossek, Schröder van der Kolk, Lockhart Clarke, Dean and Deiters, Gerlach remarks that even superficial observation of preparations carefully stained with chloride of gold shows that two sets of fibres traverse the median line, and consequently decussate. These are clearly distinct from one another, both as regards their size, their place, and their mode of decussation. The smaller set are posterior in position, and running directly across the median line constitute true commissural fibres between the two hypoglossal nuclei. The broader set proceed, like the former, from the hypoglossal nuclei, but before decussating penetrate the white substance of the brain, and occupying the most posterior part of the raphi, cross the median line and pursue their further course in the hypoglossus of the opposite side. Gerlach then proceeds to describe the hypoglossal nuclei in nearly the same terms as those employed by Dr. Clarke. Their finer structure, he states, consists of stroma or neuroglia, the nerve-cells, and nerve-fibres. The stroma belongs to the category of connective tissues, and in hardened specimens presents a finely granular aspect, comparable with that of hyaline cartilage, but when perfectly fresh it is transparent and homogeneous. That it is closely connected with the connective tissues is shown by the fact that it is continuous with the ependyma fibres of the central canal, and also with that process of the pia mater which entering the posterior furrow of the spinal cord extends to the posterior grey commissure of the spinal cord, with which it becomes directly continuous. Scattered through the neuroglia, and belonging to it, are cell-like bodies composed of a nucleus and a small quantity of investing protoplasm. The nerve-cells of the hypoglossal nucleus are large, resemble those of the anterior nerves of the grey matter of the spinal cord, have no investing membrane, but possess several processes of which one, the chief, is continuous with a nerve-fibre. In the calf, but not in man, the cells are accumulated into three heaps on each side; a median, and an anterior and posterior lateral. The nerve-fibres are similar to those of the nerve-centres generally. Some are finer, some broader; the former form an entangled network in which the finest branches are, stated by Gerlach to divide and reunite! These chiefly form the commissural fibres, the broader fibres run a straighter course and proceed to the periphery, partly by crossing to the opposite side of the median line, but chiefly by passing straight forwards and outwards on their own side. These last, though they may easily be traced back in the form of two fasciculi to the hypoglossal nucleus, he has failed to discover terminating in the cells of the nucleus. On the whole, no remarkable analogy is observable between the decussation of the hypoglossal nerves in the medulla oblongata and that of the spinal nerves in the cord.

3. M. E. Cyon, partly at the suggestion of M. Schweigger-Seidel,

undertook the examination of the peritoneum with the special view of solving certain points in connection with the nervous system, and the part which he found most appropriate for the purpose was that which extends in the frog between the abdominal parietes and the cysterna magna of the lymphatic system, since this is extremely delicate, and contains no blood-vessels, whilst it is remarkably freely supplied with nerves. This part has also the advantage of containing a few stellate pigment-cells, and in some instances, or in certain parts, ciliated epithelium, the relations of which to the nerves were thus capable of investigation. He also examined the peritoneum of rabbits and guinea-pigs. The confusion resulting from the presence of connective tissue fibres was avoided by maceration for twenty-four hours in a diluted solution (1-400) of acetic acid. The preparation was then placed for fifteen or twenty minutes in a solution of acetic acid of 1 part to 200 of water, to which 1 part of chloride of gold in 1000 of water was added, then washed with the dilute acetic acid, and finally coloured with carmine and placed in glycerine.

He found chloride of palladium and osmic acid of comparatively little value, but nitrate of silver proved serviceable, especially when the epithelium had previously been removed.

The nerve-fibres of this part of the peritoneum are doubly contoured, united in twos or threes in a special sheath. These divide, and ultimately break up into fine non-medullated fibres, which are interrupted at intervals by nuclei that produce spindle-shaped enlargements. Broad nucleated fibres are also present, which exhibit a fibrillated structure, and appear to consist of bundles of fine fibres, as is well shown in parts where the fibres are flattened out from one another, and also when one fibre becomes detached and forming a loop re-enters the primary trunk. The splitting up of an apparently simple nerve-fibre is well seen, the branches presenting spindle-shaped dilatations which must be regarded as accumulations or enlargements of the medullary sheath, and these again must be regarded as bundles of still finer fibres, or rather of fibrils. Cyon was at first inclined to believe that these primitive fibrils invariably formed loops and plexuses, since he was unable to observe any connection of the nerve-fibres with cellular elements, nor any peculiar terminal organs, but further examination taught him that there were also free extremities, though he admits the fibres were sometimes only apparently lost, and then reappeared, whilst in other cases they may have been torn across in the unavoidable stretching of the tissue in preparing it for the microscope. Still he thinks that the free extremities of the nerves play a physiologically subordinate rôle, whilst the loops are more important. Similar appearances may be seen in the peritoneum of the rabbit and guinea-pig, except that the nerve distribution is simpler and the primitive fibrils leaving the trunks are few and form wide meshed loops.

REPORT ON SURGERY.

By JOHN CHATTO, M.R.C.S.E.

On some Points relating to the Treatment of Strangulated Hernia.

—M. Tillaux, of the St. Antoine Hospital, on the whole, approves of Prof. Gosselin's decided practice in relation to strangulated hernia, viz., after performing the taxis first without and then with chloroform, without success, to immediately resort to the operation. He especially opposes Malgaigne's theory, which, by attributing so much importance to inflammation, leads to a dangerous temporisation in endeavouring to treat this. Still, M. Tillaux cannot consent to entirely overlook the old theory of obstruction or the modern one of inflammation, having so often observed, at the Bicêtre, large and old hernias partly or wholly irreducible, which had become the subjects of this obstruction, inflammation, or slight strangulation, whichever it may be, insensibly yielding to the effects of rest and cataplasms. Still, for recent hernias of moderate size and usually reducible, which have become strangulated, M. Gosselin's rule is the proper one.

The taxis, when it succeeds, acts as if by enchantment in the relief of symptoms; but numerous questions arise in its application, such as whether it should be moderate or forcible, how long it should be continued, and at what stage of the strangulation it ceases to be practicable. Thus, while M. Gosselin employs all the force that can be excited by two, four, or even six hands, most surgeons think a more moderate procedure preferable; and few would carry their enthusiasm for it so far as M. Thiry, of Brussels, who perseveres with it for twelve or fifteen successive hours. M. Tillaux is of opinion that when it has been properly applied for a quarter of an hour upon a patient in a state of anæsthesia, and does not succeed, the operation should be resorted to. The surgeon is not in possession of any positive sign that the taxis will prove harmless, and M. Tillaux cites a recent case in proof of this. In a patient who presented an inguinal hernia of twenty-four hours' duration, with the skin tense over it, and sound in appearance, and unaccompanied by any sign of local reaction, he performed a moderate taxis for a few minutes, the hernia being reduced. Two hours after the patient died in intense pain, and the autopsy revealed intestinal perforation. How little the mere duration of the strangulation can provide a rule may be seen from another case, in which a femoral hernia had been strangulated for eight days, and yet the intestine was found perfectly healthy.

After adverting to the ordinary rules for the operation, according to the condition in which the intestine is found, M. Tillaux adverts to the rarer case in which the lesion of the intestine is limited to its serous membrane. In a femoral hernia, in which strangulation had

continued sixteen hours, he found the peritoneum detached from the intestine to about the extent of four square centimètres, just as the epidermis is in a burn. As the muscular coat appeared healthy, he returned the intestine, and the patient recovered after some slight symptoms of localised peritonitis.

Besides the ordinary mode of strangulation by the rings and the orifices of the cribriform fascia, M. Chassaignac has pointed out one due to the inflexion of a noose of the intestine over a sharp edge, as, for example, Gimbernat's ligament. In this case the taxis only increases the inflexion of the hernia, instead of reducing it, all pressure acting, in fact, directly on the hernia. In a case here selected, in which the taxis had been freely employed prior to admission, the noose of intestine, lying with its convexity inwards, was strangulated by the external edge of Gimbernat's ligament. It was healthy, and having been straightened in order to be returned, it spontaneously re-entered the abdomen without any pressure whatever having been exerted upon it. The patient died of peritonitis.

M. Tillaux protests against the adoption of the practice recently advocated by M. Girard of performing the operation, and leaving the hernia unreduced, under the idea that the reduction of the inflamed intestine is the cause of the peritonitis that so often ensues. M. Girard bases his recommendation chiefly on two cases, in which, owing to gangrene in adhesions, reduction had not taken place, and yet the patient recovered. This, M. Tillaux observes, is the old story of their recovery, in spite of, and not in consequence of non-reduction; and he cannot treat the proposal as serious until cases have been treated by intentional non-reduction. He observes the return of the intestine is not the source of danger in herniotomy, but the peritoneal wound; and he believes that ovariectomy may teach us one reason why operations for hernia are so fatal. In it the surgeon, above all things, is assiduous in preventing the sojourn of a drop of blood or other fluid in the cavity of the peritoneum, for it is from the subsequent decomposition of this that peritonitis is to be feared. In hernia it is obvious that the blood from the bleeding surfaces easily may gain access to the cavity. This is a mere hypothesis, but it is desirable that the attention of surgeons should be turned in this direction.—*Bull. de Thérapeutique*, June 30th.

On Internal Otitis in New-born and Young Infants.—A short time since (*vide* 'Medical Times,' June 5) M. Parrot, surgeon to the Hôspice des Enfants, Trouvés, read an interesting paper to the Paris Hospital Medical Society on this subject, drawing attention to the great frequency with which the affection is met in autopsies of infants. In the present communication, MM. Baréty and Renaut, two of his pupils, furnish a complete macroscopic and microscopic account of the lesion. They observe that prior to M. Parrot's communication internal otitis of infants had been unnoticed in France and England, except, and then very imperfectly, as due to an exanthematic origin. In fact, Prof. von Tröltsch and his American translator, St. John Roosa, are the only writers who have noticed

the existence and frequency of purulent catarrh of the tympanum in children, together with the influence attributable to it in the production of deaf-dumbness. According to their own experience, derived from the performance of a large number of autopsies in 1868 and 1869, the lesion is one of very frequent occurrence. Indeed, the absence of some of the different stages of otitis was quite exceptional, a fact somewhat explained by M. Parrot's observation, that the subjects of the lesion had usually suffered from disturbance of nutrition, owing to insufficient or faulty food. Whatever their cause, MM. Baréty and Renaut describe minutely the various appearances of which Von Tröltsch had only as yet given a rough account, as witnessed by the naked eye. This description, which is illustrated by drawings, we have no space to follow, and can only refer to it those interested in a good anatomical account of internal otitis. The authors, from their clinical observation of these cases, quite agree with Parrot and Von Tröltsch, but the lesions are only one out of numerous other signs of an intense impoverishment of the economy which are met with, especially during the two first months of life, its subjects also often being born prematurely.—*Archives de Physiologie*, May.

On Reduction of Dislocation of the Humerus.—Professor Podratzki observes that, while as a general rule recent dislocations are reduced without difficulty, cases are met with which resist the usual methods, however skilfully employed. In old dislocations, especially when the use of chloroform is contraindicated, such resistance is, of course, much more frequently met with. He wishes to direct attention to the two methods of reduction which have been recently introduced, having himself derived great advantage from their employment. The first is the "pendulum" method, described by Professor Simon, of Rostock ('Brit. and For. Med.-Chir. Rev.,' Jan., 1867, p. 269), which he has found to succeed with an ease and rapidity that has surprised him. It consists essentially in raising the patient by the dislocated arm, converting his body, in fact, into an agent of extension. This is done by an assistant employing a towel, or, in obstinate cases, a pulley, while the operator directs the head of the bone into its cavity. In a case in which the dislocation had taken place eight weeks before, reduction was thus effected immediately, without the employment of chloroform. The other method is that of Schinzinger, and consists in forcibly rotating the humerus outwards, the shoulder-blade being fixed by an assistant. Such an amount of force may be exerted by the forearm converted into a lever, that there is some danger of breaking the bone. Rotation has been employed by Syme, Dumreicher, and others, but not so methodically as by Schinzinger. In the three recent cases in which Prof. Podratzki has resorted to it the success was remarkable for its rapidity, although in two of these no chloroform was given. He believes the plan unsuited for old dislocations, which can be more efficaciously and safely treated by the pendulum method.—*Wochenblatt der Gesell. der Aerzte in Wien*, May 26th and June 9th.

Discharge of a large Calculus by the Perineum.—M. Vast, as the result of a review of the literature of the subject, observes that this shows—(1) That very large urinary calculi may sometimes exist for years without causing much pain or injury to health, or inducing surgical aid to be sought for; (2) at a given moment these calculi, although large, may become urethral; (3) that the accidents induced by these foreign bodies, formidable in appearance, may in healthy subjects have a favorable issue; (4) the ensuing urinary fistulæ may under rational treatment become rapidly obliterated.

M. Vast then details a very interesting case which has occurred in his own practice, as highly illustrative of the above conclusions. The patient, forty-five years of age, had always enjoyed good health, following the arduous employment of an agricultural labourer. When about twenty he hurt the perineum over a chair, and has since then often had severe pain in that locality, and sometimes has found micturition difficult and the urine sanguinolent. At the beginning of last June, after a laborious day, he was seized with severe pain in the perineum, and having in three or four days sought assistance, he was found with the perineum swollen, hard and red, being in excessive pain. The swelling went on increasing, and in a few days large gangrenous destruction of the soft parts took place. On exploring the wound a calculus of somewhat elongated form was found at its posterior parts. This was extracted without much difficulty by means of a polypus forceps. The gangrenous mass which had resulted from the infiltration of urine rapidly cleansed, the wound cicatrising almost without the aid of art. In five or six weeks from the rupture of the urethra the patient had completely recovered, no trace of a fistula remaining. The calculus, comprised entirely of phosphate and oxalate of lime, was of a pyriform shape, measuring $5\frac{1}{4}$ centimetres in length, and $3\frac{1}{4}$ in its largest circumference. It weighed 49 grammes, *i. e.* $12\frac{1}{4}$ drams, so that it ranks with large calculi. M. Vast mentions several reasons for believing that it was originally a vesical calculus.—*Bulletin de Thérap.*, Nov. 15th.

On Endemic Hemeralopia.—M. Galezowski having been called on to advise concerning several cases of hemeralopia which had occurred in the 64th Regiment of the line, and believing, as the result of ophthalmoscopic examination, that a spasmodic contraction of the arteries of the retina constituted the first stage of the pathological process, he determined to try the effect of the calabar bean, and prescribed a collyrium containing two centigrammes of eserine to ten grammes of water, dropping four drops one or more times in the eye daily. This effected a cure more rapidly than any other means in several cases in which it was tried. Upon the subject of hemeralopia in general, M. Galezowski's investigations have led him to these conclusions—(1) Endemic hemeralopia is an affection of the nervous membrane, characterised by a spasmodic contraction of the arteries of the retina and its consecutive anæsthesia. In certain forms of the disease there are found greyish-white serous infiltrations, especially at the circumference of the papilla, as has been

demonstrated by MM. Quaglino and Martialis, and as I have verified during this year on some of my patients. (2) This affection is generally endemic, and is observed in countries and localities where marsh fevers exert their ravages. It is also very often met with among sailors and soldiers who bivouac in humid localities. (3) Endemic hemeralopia is usually cured speedily, but it leaves a disposition to relapse in spring and autumn. M. Perreon, surgeon of the 64th Regiment, has found the same soldiers seized with hemeralopia during several successive springs. (4) The hemeralopia attacks both eyes at once, and in the same degree. (5) This form of amblyopia may be simulated by soldiers, and detection may be attended with considerable difficulty, when ophthalmoscopic phenomena are absent, as happens sometimes. Shutting them up in a completely dark room for a day or two easily induces them to abandon the attempt at deception. They become wearied out, and declare that they are cured. (6) The internal use of cod-liver oil, and fumigation by the vapours of the livers of animals directed on to the eye, have no efficacy whatever in the disease. More success is obtained from the use of purgatives and emetics, as the affection is often connected with gastric disturbance. (7) The hemeralopia which accompanies congenital or syphilitic pigmentary retinitis is permanent, while this form is transitory. The one progressively leads to complete blindness, while the other is usually followed by recovery.—*Gaz. des Hôp.*, October 23rd.

On the Value of Extirpation of the Calcaneum.—Professor Polaillon, after giving an analysis of the recorded cases and a description of the operation, thus sums up his opinion as to its value and indications :— (1) Extirpation of the calcaneum ought to be accepted in practice, as it allows of the preservation of a foot that is very useful in standing and walking. (2) Its success varies with the age of those operated on. In children and adolescents it succeeds in eight out of nine cases, furnishing excellent results as regards the ulterior employment of the limb. In the adult it has failed in half the cases, and when it succeeds its results are less excellent, the foot accommodating itself ill to the loss of the calcaneum at the age when the period of growth of the skeleton is completed. (3) Amputation is, therefore, better suited to adults than extirpation, a stump generally furnishing a better point of support than the foot deprived of the calcaneum. (4) It is to be borne in mind, however, that amputation is a far more fatal operation than extirpation, and that under certain conditions this operation may be followed by a cure with far less peril to life. (5) When extirpation has to be determined on the greatest pains should be taken to ascertain that the calcaneum is alone diseased, and that the neighbouring parts and the tibio-tarsal articulation do not participate in the affection. But when this diagnosis is difficult and uncertain, the operation need not be necessarily abandoned. An exploratory incision may be made down to the bone in such a manner that it may afterwards serve, according to the extent of the disease, either for the removal of the calcaneum, for tibio-tarsal disarticulation, or an amputation of the leg. (6) In com-

minutive fracture of the calcaneum from gunshot or other cause, with a penetrating wound of the integuments, extirpation will probably be necessary, as the osteitis will generally go on to caries. (7) Central caries, which has not yet attained the articular surfaces of the calcaneum, indicates the operation of complete gouging. Peripheric caries, with suppuration of the articular surfaces, requires the disarticulation of the bone and the scraping or cauterisation of the corresponding articular surfaces. But if the caries has penetrated deeply into the astragalus and other bones, amputation should at once be performed. (8) Complete necrosis and diffuse phlegmonous periostitis are the affections in which extirpation best succeeds, as the periosteum being detached by the suppuration, the operation is reduced to the simple removal of a sequestrum. These are the cases, too, in which the best bony reproductions are met with. (9) The morbid conditions which indicate extirpation of the calcaneum are especially enchondroma and fibroma, hardly ever cancer. At its commencement cancer does not form a tumour which is sufficiently appreciable for diagnosis, and at a later stage it has invaded the surrounding tissues, and no other operation than amputation of the entire foot is prudent. (10) The presence of fistulous openings and small ulcers, and inflammatory induration of the skin of the heel, form no contraindications to extirpation. But ulceration of the skin over a large extent, its invasion by a cancerous tumour, or even its excessive thinning through the development of a non-malignant tumour, are positive contraindications to the operation. Even supposing healing to take place, walking would have always to be performed on a cicatrised surface, and would give rise to various accidents. (11) In all cases advantage results from not delaying the operation for too long. (12) The traumatism resulting from it is inconsiderable, allowing it to be performed on subjects even when exhausted by disease.—*Archives Générales*, Sept. and Oct., 1869.

SUMMARY.

Amputation.—Caradec. Supra-malleolar A. (*Gaz. des Hôp.*, Sept. 21.) Case related illustrating the advantages of the elliptical procedure of Prof. Duval of Brest.—Berenger-Féraud on Sutures of the Bones in Tibio-tarsal A. (*Bull. de Thérap.*, Sept. 30, Oct. 15.)

Aneurism.—Heine. On Cirroid A. of the Head (*Prag. Viertel.*, B. iii and iv. An elaborate memoir on what the author prefers terming *angioma arteriale racemosum*. He relates a case at length, and enters into a critical examination of sixty published examples of the disease).—Fischer. Digital Compression and Flexure in A. (*Ib.*, B. ii, iii, and iv. An exhaustive analysis of published cases, but no new facts added.)—Rouge. Successful Case of A. of the Common Carotid treated by Intermittent Digital Compression during seventeen days. (*Bull. de la Soc. de Chir.*, 1868. With a Report by Paulet.)

Arteries.—Kocher. Two Cases of Ligature of the Common Femoral. (*Langenbeck's Archiv*, B. xi, H. 2).—C. Schmidt. Suc-

cessful Case of Ligature of the Subclavian. Berlin Klin. Woch., Sept. 20.)

Club-foot.—Tufnell. Treatment of C. by Direct Extension. (Dublin Journ., Nov. Woodcuts showing the mode of employing adhesive plaster for this purpose.)—Lucke. Employment of Elastic Traction in Deformities of the Foot. (Berlin Klin. Woch., Oct. 11. Woodcuts of the apparatus.)

Dislocations.—Blasius. Traumatic D. of Spine. (Prag. Viertel., 1869, b. cii—civ.)—Cruise. On Extension by means of an Endless Screw Windlass. (Dublin Journ., Nov. Additional experience confirmatory of the good opinion of the mode published in 1861. Illustrations.)—Bevan. Rare form of D. of the Acromial End of the Clavicle Upwards and Backwards. (Ibid., with illustrations.)—Sistach. Clinical Examination of Traumatic Luxations. (Gaz. Méd., June 26—Nov. 27. An elaborate account of fourteen of the cases of dislocation which occurred at the Military Hospital, Constantine Algeria, in 1866 and 68.)

Ear.—Jacoby. Cases of Intra-auricular Morbid Growths treated by Galvanocautic. (Archiv f. Ohrenheil, 1869, H. 1. Three cases successfully treated.)—Zaufal. On *Otitis Media Serosa*. (Ibid. Several cases, both in the acute and chronic form.)

Excisions.—Hannover. The Ultimate Results of the E. made in the Danish Army in 1864. (Jahrb. d. Gessell d. Wiener Aerzte, 1869, H. 4. These were for the most part unfavorable.)

Eye.—Swaney. Case of Essential Phthisis Bulbi. (Dublin Journ., Nov.)—Gavarret. Report on Perrin and Mascart's Optometer. (Bull. de l'Acad. de Méd., July 6.)—Speer. Case of Embolism of Central Artery of Retina. (American Journ. of Med. Sci., Oct.)—Jeffries on Congenital Cataract in Children Simulating Near-Sightedness. (Boston Med. and Sur. Journ., Oct. 7. Attention called to the frequency of this and the danger of overlooking it.)—Phillips. On a new mode of Operating for Ectopion. (Union Méd., Nov. 9.)—Preuss. On Epidemic Granular Conjunctivitis. (Berlin Klin. Woch. Oct. 11—25).—Von Graefe. On the Operation for Dynamic Divergent Strabismus in relation to Progressive Myopia. (Zehender's Monatsblatt, Aug.)—Daguenet. On Alcoholic Amblyopia. (Annales d'Oculistique, Sept.)

Fractures.—Phillips. Case of F. of Neck of Femur successfully treated by weights applied in the axis of the limb and the axis of the neck. (American Journ. of Med. Sci., Oct. Woodcuts.)—Bullock. An Improved Apparatus for F. of the Lower Jaw. (Ibid. Woodcut given of the instrument, which the author states to be a great improvement on that of Lonsdale.)—Podratski. Case of Ununited F. of the Femur. (Wien. Med. Woch., Aug. 18—28. Treated by excision of ends of the bones and fixing them with steel screws. Fatal issue.)—Sonrier. Two Cases of F. of the Meatus Auditorius by Contrecoup. (Gaz. des Hôp., Oct. 26.)

Hernia.—Uhde. On the Results of Operations in Umbilical H. (Langenbeck's Archiv, B. xi, H. 2. An analysis of 188 published cases.)—Wernherr. On Fatty H., and on Fatty Tumours Simulating H. (Virchow's Archiv, July and Aug.)—Englisch. On the

Mode of Formation of Internal H. (Jahrb. d. Gessel d. Wien. Aerzte Nov. 4.)—Richardson. On a Hernia Knife, with Broad Back for Protecting the Bowel. (Dublin Journ., Nov.)

Joints.—Hoppe. Treatment of Chronic Inflammation of J. (Deutsche Klin., June 26, Sept. 4)—Koster. Fungous Inflammation of J. (Virchow's Archiv, Sept. Five cases given with illustrations of the microscopical anatomy.)

Laryngoscopy.—Navratil. Paralysis of the Glottis. (Berlin Klin. Woch., Sept. 6—20. Woodcuts.)—Stoerck. Laryngoscopic Operations. Wien. Med. Woch., Sept. 29, Nov. 13. Woodcuts.)—Navratil. Polypus of Larynx removed by Laryngo-fissure. (Ibid., Sept. 8. Woodcuts.)—Fieber. Foreign Body in Larynx removed by aid of the Laryngoscope. (Ibid., Oct. 27.)

Ovariectomy.—Boinet. Case of O. practised successfully for the second time. (Gaz. des Hôp., Oct. 2.)—Brandt. Successful Case at Klausenburg. (Wien. Med. Woch., Sept. 15 and 18.—Bromeissl. Case of O. performed by Prof. Braun in Vienna. (Ibid., Oct. 13 and 20. Fatal from peritonitis.)—Liègeois. Two Cases. (Gaz. des Hôp., Dec. 4. One performed on a woman, æt. 63, with success; the other on a girl, æt. 17, fatal.—Péan. Successful Case. (Gaz. Méd., Dec. 11. The uterus being found diseased was entirely removed.)

Phimosis.—Cruise. Treatment of P. by Sudden Dilatation. (Dublin Journ., Nov. Effected by a dilating forceps, of which a woodcut is given.

Plastic Surgery.—Buck.—Case of Restoration of the Lip by Plastic Operation, followed by Esmarch's Operation on the Jaw. (American Journ. of Med. Sci., Oct. Illustrations.)

Staphylorrhaphy.—Herzog. On Staphylorrhaphy. (Allg. Wien. Med. Zeit., Sept. 7—Oct. 26. A review of operative procedures in relation to a successful case by Prof. Ritter v. Pitha.)

Syphilis.—Diday. On Mixed Chancroid. (Lyon Méd., Nov. 21.)—Solovveitschik. On Syphilitic Disease of the Skull. (Virchow's Archiv, Sept. and October. A minute description of the appearances in nine cases of otitis and gummy caries of the cranium, which occurred at the Odessa Hospital.)—Monti. Treatment of Congenital S. by Sublimate Injections. (Jahrb. f. Kinderh., H 4. Found in fourteen cases to effect a cure very rapidly in children, but were frequently followed by severe local irritation.

Urinary Infiltration.—Menzel. Experimental Inquiry into the Influence of Urine on the Cellular Tissue. (Wien. Med. Woch., Oct. 9—23. The chief result of these experiments on dogs is to show that acid urine has little tendency to induce gangrene, as compared with alkaline.

Varicocele.—Morgan. Treatment and Cure of V. by Suspension of the Testis. (Dublin Journ., Nov. An illustration of the suspender given.)

Vesico-vaginal Fistula.—Watson. On Harelip Suture in. (Ed. Med. Journ., Oct.)

REPORT ON MIDWIFERY.

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I.—THE NON-PREGNANT STATE.

1. *A Case of Death from Peritonitis following an Intra-uterine Injection.* By Dr. v. HASELBERG.
2. *A Temporary Dilatation of the Fundus of the Vagina.* By Prof. ED. MARTIN.
3. *Fibroid of the Uterus; repeated hæmorrhages; transfusion; recovery.* By Dr. GENTILHOMME.
4. *The Sero-plastic Method of Ovariectomy.* By Dr. MASLOWSKY.
5. *Cases of Ovariectomy.* By Dr. SVEN SKÖLDBERG, of Stockholm.
6. *A new Method of Puncturing the Ovarian Cyst during Ovariectomy.* By Dr. HOFMOKL.
7. *Ovary and Fallopian Tube the Contents of an Incarcerated Crural Hernia.* By v. OETTINGEN.
8. *A Radical Operation for the Relief of Retroflexion and Retroversion of the Uterus.* By Dr. SCHETELIG.
9. *Improved Pessary for Flexions of the Uterus.* By JOHN CLAY, Esq.
10. *Retroflexion of the Uterus in a Single Woman caused by lifting a heavy weight.* By Dr. HALL DAVIS.

1. Dr. v. Haselberg reports a case of death from peritonitis consequent upon the injection of perchloride of iron into the uterine cavity. The case is so completely observed, and so important, that it calls for full reproduction. A puella publica, 32 years old, having had an abortion at six months some time before, came under treatment with antelexion of the uterus to such an extent as to render the passage of the sound difficult. The vaginal portion was enlarged, and an easily bleeding ulcer existed on the anterior lip, extending into the cervical canal. The sore was touched with nitrate of silver. Profuse bleeding followed, partly from varicose vessels surrounding the sore, partly from the uterine cavity; and even after the ulcer was nearly healed by the use of alum-plugs profuse menorrhagia occurred. It was therefore determined to try injection of perchloride of iron, but it was only after repeated trials that at last the syringe was got beyond the point of flexion of the canal into the cavity of the uterus. Immediately after the injection the patient suffered no pain, but in the night a severe rigor, with consecutive sweating, occurred. This was repeated every night, notwithstanding the use of quinine.

The patient quickly collapsed, even before signs of peritonitis set in. On the fifth night rigor was accompanied by severe vomiting, and immediately abdominal pain followed. In the following night, whilst vomiting, terrible pain suddenly came in the abdomen, whereupon she fainted and died.

Autopsy.—The abdomen was much distended. Being opened a large quantity of stinking gas escaped. The intestines were united

by recent exsudation; the lower parts of the peritoneal cavity were filled with stinking pus of greyish colour. The source of this matter was discovered in a cyst of the right ovary, the size of a fist, whose walls had fallen in, and out of which, by pressure, matter of like appearance could be squeezed through a small opening. On the upper surface of the cyst the opening of the right Fallopian tube was found. From this a large sound could be passed the whole length of the tube into the uterine cavity. The mucous membrane of the uterus was stained pitch-black, as if with ink, and the same colour extended along the right tube to its outer extremity. The mucous membrane of the left tube was slate-grey. Pieces were taken from the uterus and the right tube, and were seen to be pitch-black for a depth of $\frac{1}{2}$ to 1 millim. Chemical reaction showed in these parts a copious infiltration with iron. Small pieces of the surface were steeped in weak hydrochloric acid, whereupon the black colour disappeared. Prussiate of potash added to this solution caused a red colour, and sulphide of ammonium added to the neutralised solution caused a turbid black-grey colour.

V. Haselberg then discusses the features of the case and the precautions to be adopted in intra-uterine injections. He has derived the most signal advantages from the practice. Opinions differ widely as to the possibility of injected fluids passing along the tubes into the abdomen. The experiments of Hennig and Klemm on the dead body prove how difficult it is under normal conditions to inject through the tubes, even with closure of the os uteri internum, a great quantity of fluid, and considerable pressure; and in the living subject the probability must be still less, since these three conditions do not exist. It is true, nevertheless, that the conditions in the living are not known, and Hildebrandt and Matthews Duncan describe an abnormal opening of the tubes to such an extent that even the point of the syringe could be introduced. In the case under discussion such a dilated condition of the tube existed. He quotes Gaillard Thomas ('A Practical Treatise on Diseases of Women,' 1868) as giving a case in which fluid passed along the tube, and Mandl ('Wien Med. Presse,' 1869) as saying that this passage easily and often takes place, and that even a drop of water in the tube will cause the most acute pains. Other injuries may be caused by the mere introduction of the syringe, as laceration of the uterus, tearing of peritonitic adhesions, hæmatoceles. And there are puzzling cases of simple vaginal injections followed by pain, convulsions, and fatal peritonitis.

V. Haselberg does not think the preliminary dilatation of the uterus so important a safeguard as others do. He thinks it more important to suck back into the syringe the superfluous fluid injected. But in spite of all precautions he cannot be sure that pains or perimetritis will not arise. (In the case under discussion the cause of death was clearly the peritonitis; the immediate cause of the peritonitis was the rupture of the ovarian cyst and the escape of its foul contents into the peritoneum. It is possible that the severe vomiting caused the rupture of the cyst, so that the injection cannot be regarded as the direct cause of death. The state of the right

tube, however, proves that in some cases fluids injected into the uterus may traverse the tubes. This danger may certainly be lessened by previously securing full dilatation of the cervical canal, and using a swab instead of the syringe.—R. B.)—*Monats. f. Geb.*, 1869.)

2. Professor E. Martin describes a condition of the vagina which is observed under particular circumstances. It consists in a temporary dilatation of the fundus, not the result of stretching or distension, but which is caused by a pathological action of the neighbouring ligaments, that is the pubo-vesico-uterine, and the sacro-uterine, the muscular bundles of which contract. The examining finger finds the roof of the vagina so wide that it seems as if its walls were applied close to the sides of the pelvis. This condition is found when there is hæmorrhage with uterine colic, and in secondary puerperal hæmorrhage. In such cases the os uteri is open, and the roof of the vagina seems higher than usual. Under the use of means to arrest the bleeding, this dilatation disappears completely in twenty-four hours. Dr. v. Haselberg says, speaking on the subject, the dilatation takes place under the effort of the uterus to empty itself.—*Monats. f. Geb.*, 1869.

3. Dr. Gentilhomme relates a case of remarkable interest. A woman was delivered of her second child in May, 1864. At the beginning of 1865 she had a profuse flooding, which was renewed at the next menstrual epoch. A fibroid the size of a fist was found. The bleedings were repeated, so that extreme anæmia was induced, attended with the most threatening symptoms. Transfusion was determined upon. A healthy woman supplied 125 grammes of blood; the apparatus of Moncoy was used. During the first moments of the operation nothing remarkable was observed, but when the stated quantity of blood had been injected the patient, whose cheeks had become red, cried out, "I am suffocating." Symptoms of asphyxia, thumping of the heart, general shivering, pains in the chest and loins, falling of the pulse which had previously risen, general coldness, vomiting, and great thirst set in. These symptoms lasted two and a half hours, then gradually subsided; the breathing became easier, the pains went away, the pulse became strong and regular—120, warmth returned. From this time she gradually mended, taking soup, chicken, and wine. Some time afterwards symptoms of peritonitis appeared, and pus flowed from the uterus. It was found that the fibroid tumour had been cast into the cavity of the uterus, whence it was expelled spontaneously, hanging only by a slender stalk, which was easily separated. It weighed 140 grammes.

The author does not think the threatening symptoms attending the transfusion were caused by either air or clots.—*Gaz. Hebdomad.*, 1868.

4. Dr. Maslowsky describes a new method of dealing with the stump in ovariectomy, which he calls the seroso-plastic. The ovarian tumour being drawn through the wound, he applies the clamp, then dissects off from the stump or lower part of the tumour the serous investment, so as to obtain a semi-circular flap. He thus cuts through the pedicle with a knife immediately above the flap, so

that the flap keeps in relation with the remains of the pedicle; he cauterises with the iron the bleeding vessels; then covers the cauterised parts with the serous flap, and unites the edges with silver sutures to the hinder part of the peritoneum. The clamp is then removed, and the stump is left in the pelvis. The advantages, he contends, are—its general practicability, the security it gives against secondary hæmorrhage, the absence of any parts which can undergo necrosis, and thus the removal of the chief cause of septicæmia. The serous flap quickly heals, so that the abdominal wound may close by first intention.—*Centralbl. f. d. Med. Wissensch.*, 1868.

5. Dr. Sköldberg, who studied some time in London, publishes a table giving the results of his experience in ovariectomy. Table I contains twenty cases of completed ovariectomy, giving seventeen recoveries and three deaths. In all the pedicle was treated by cautery, in some ligature being used as well. Table II contains four cases, in which an exploratory incision was made, extirpation of the tumour being found impossible. One of these cases died; there was an ovarian cyst and cancer surrounded by ascitic fluid. Table III contains one case only. There was a solid ovarian tumour diagnosed before the operation; it was found to be cancer of the right ovary, the size of an adult head, and surrounded by ascitic fluid. The patient died in twenty hours.

6. Dr. Hofmokl recommends a new plan of tapping the cyst when performing ovariectomy. Having selected the place for the puncture, he transfixes the cyst with three slightly curved needles so arranged as to enclose a triangular space; a string is then run loosely round these needles, and the trocar is plunged in the centre space. After partial evacuation the thread is drawn up, so as to fix the cyst-wall tightly round the canula.—*Wien. Med. Presse*, 1868.

7. Dr. v. Oettingen relates a case of a woman, æt. 41, with a crural hernia, in which an ovary was diagnosed. On account of peritoneal symptoms the sac was opened, and the ovary and Fallopian tube were returned into the abdomen.—*St. Petersb. Med. Zeitschr.*, 1868.

8. Dr. Schetelig describes a remarkable operation performed by Koeberle for the relief of retroflexion of the uterus. A woman suffered much after her first labour. Retroflexion was discovered. Koeberle proposed to fix the fundus of the uterus to the anterior wall of the abdomen by operation. This was carried out on the 27th March, 1869. Incision was made as for ovariectomy; the finger carried through the opening brought the fundus uteri forward from Douglas' pouch. An ovary and its related tube were united to the lower angle of the abdominal wound. Recovery was uninterrupted.—*Centralbl. f. M. Wissensch.*, 1869.

9. Mr. Clay describes a new pessary for the treatment of uterine flexions. It is a small inelastic ball secured to the fundus of an ordinary elastic pessary, so that its axis forms an obtuse angle with that of the larger part of the pessary. For anteversion the small portion of the pessary is passed in front of the cervix. In retroversion it is passed behind.—*Lancet*, 1869.

10. Dr. Hall Davis relates a case in which retroflexion of the uterus

was caused by lifting a heavy weight. It was successfully treated by keeping the bowels clear, by restoring the uterus to its normal position by the sound, keeping the patient prone on her stomach, and cold water injections.—*Lancet*, 1869.

II.—PREGNANCY.

1. *Memoir on the Utricular Glands of the Uterus, and on the Glandular Organ of New Formation, which is Developed during Pregnancy in the Uterus.* By Prof. ERCOLANI.
2. *Remarkable Case of Extra-uterine Abdominal Pregnancy.* By Dr. LECLUYSE.
3. *Retention of Fœtus and Placenta in Utero for Eighteen Months.* By Dr. M'MAHON.
4. *On Pregnancy, considered as a Contra-indication to Prolonged use of Arsenic.* By Dr. DU VIVIER.
5. *Case of Inversion of the Uterus.* By JAMES BLAKE, M.D.
6. *Five Cases of Rupture of the Uterus.* By Dr. E. WHITTLE.
7. *On the Operations for the Relief of Chronic Inversion of the Uterus, with the History of a Case Successfully Treated by a New Method.* By ROBERT BARNES, M.D.

1. Ercolani contends that the maternal portion of the placenta of the vertebral mammifera, including the human species, is always glandular. It is a glandular organ of new formation, destined to secrete a humour serving for the nutrition of the fœtus. The placenta is formed of two distinct parts—the foetal, which is vascular and absorbent; the maternal, which is glandular and secreting. He says the human placenta belongs to the same order as that of the ape.

2. Dr. Lecluyse relates a very remarkable case of extra-uterine gestation following on Cæsarian section. A woman, æt. 28, had undergone this operation on account of contraction of the pelvis, and became pregnant the next year. Abdominal pains set in, the cervix-uteri could not be reached, the belly was pendulous, and the fœtus was felt through the walls; gastrotomy was postponed for four days, then foetal movements had ceased. An incision was made very carefully, but so thin were the abdominal walls that the amniotic sac was pierced at once. A fœtus of seven or eight months was extracted dead. Gentle tractions on the cord showed that the placenta was adherent to the anterior and inferior surface of the small intestines. It spread out star-shaped, losing itself by vascular irradiations amongst the intestines. It was ascertained that the ovum had no other envelope than the amniotic membranes. The wound was closed, except at the lower part, for the exit of the cord; the placenta being left. On the fifth day the placenta was decomposing, a portion was cut away; fetid discharge set in. Peritonitis and obstinate vomiting appeared, and death followed on the tenth day.

Autopsy.—The intestines showed no trace of the placental insertion. But the singular feature was that the uterus, the size of a goose's egg and contained in the right iliac fossa where it had contracted adhesions, presented on its anterior surface an oblong opening communicating with the uterine cavity. This was the result of

the Cæsarian section, which had not closed. No doubt the ovum had passed out through this into the peritoneal cavity to fix upon the intestines.—*Bull. de l'Acad. de Méd. de Belgique*, 1869.

3. Dr. McMahon relates a case in which a foetus of four months' development, with its membranes, was retained in utero for eighteen months. When four months pregnant the patient seemed about to miscarry, there being copious flooding and pains. All went off, and another attack of hæmorrhage and pain came on two months later. She then went on well for a whole year, when a placenta moulded by compression and enclosing a shrivelled four months' foetus was expelled. The author thinks, from the size of the placenta, that it had continued to grow some time after the first symptoms of abortion.—*Pacific Med. and Surg. Journ.*, 1869.

4. Dr. Du Vivier treated two women, pregnant, for psoriasis with arsenic. Both aborted at seven months of dead children. He believes this was caused by the arsenic. The women bore the dose, an ordinary one, perfectly well. Dr. A. Guérin had observed similar consequences from giving mercury.—*Annales de Dermatologie*, 1869.

5. Dr. James Blake relates a case of inversion of the uterus presenting some features of interest. A woman in her first labour had the placenta dragged away, the uterus becoming inverted. Efforts to return at the time failed. Two days later further attempts were made under chloroform, also without success. Dr. Blake was then sent for, but concluded that it would be better to postpone the taxis until the third week, when involution should be more advanced. At the end of three weeks he found the uterus the size of a turkey's egg. He reduced it in thirty minutes by pushing the points of three fingers into the sutures formed where the cervix grasped the uterus, and stretching them out so as to stretch the neck, whilst the body of the uterus lay in the palm.—*Pacific Med. and Surg. Journ.*, 1869.

6. Dr. Ewing Whittle relates five cases of rupture of the uterus which have come under his own observation. In one case rupture was inferred, apparently on good grounds, but the patient recovered; and in another case no post-mortem examination was made. All the subjects were pluriparae, and in all there was positive or presumptive evidence of degenerate tissue. The author submits that in cases where the progress of labour is slow, and we have to fear that the uterus is feeble and flabby, we should soothe by opiates until the uterus is dilated, then rupture the membranes, and deliver if the labour still linger.—*Liverpool Med. and Surg. Reports*, 1869.

7. Dr. Barnes, in a memoir on chronic inversion of the uterus, defines "recent" inversion as terminating with the completed involution of the organ; he analyses the results of the different methods of treating chronic inversion, showing the great mortality and risk attending the various modes of amputating, the dangers of forcible taxis, and the successful results of sustained elastic pressure. He then describes a case which, having resisted sustained elastic pressure, was reduced after incising the constricted neck of the uterus, the patient making a good recovery.—*Med. Chir. Transactions*, Vol. lii, 1869.

III.—LABOUR.

Case of Labour, complicated with Pilo-cystic Tumour; Extirpation of the Tumour; Menstruation through the Wound; Recovery.
By R. W. GIBBES, M.D.

Dr. Gibbes relates a remarkable case in which labour being terminated by the forceps on account of syncope, the patient was harassed by the most intractable after-pains. A tumour was discovered above and behind the pubes, distinct from the uterus, and moveable. On a subsequent day this tumour was felt per vaginam in the anterior cul-de-sac. It then increased rapidly to the size of the largest shaddock, and it was considered necessary to remove it. This was done as by the operation for ovariectomy. It grew from the left broad ligament. About three inches of the Fallopian tube was included in the ligature. The cyst contained pus and a mass of fine black hair. Menstruation occurred at several successive monthly periods through the wound. She ultimately recovered.—*Amer. Journ. of Med. Sci.*, 1869.

IV.—THE CHILD.

The Normal Temperature of Children. By Dr. J. FINLAYSON.

The normal temperature in children has been investigated by Dr. James Finlayson. He had observed that during convalescence from several diseases the evening temperatures frequently went down to a point which he was disposed to regard as abnormally low, but as they continued low even when the child had completely recovered, it seemed important to ascertain what the normal temperature really was, and what daily variation occurred in the healthy subject. His conclusions are—(1) That the daily range of temperature is greater in the healthy child than that recorded in adults; that is, he got a mean range of between 2° and 3° F. (2) There is invariably a fall in the evening amounting to one, two, or three degrees. (3) The most striking fall occurs between 7 and 9 p.m. (4) The temperature usually begins to rise between 2 and 4 a.m. (5) Fluctuations between 9 a.m. and 5 p.m. are usually trifling. (6) There seems to be no very definite relationship between the frequency of the pulse and respirations, and the amount of normal temperature.

Dr. Finlayson observes that high evening temperatures are the rule in cases of tubercular and enteric fever. Hence a persistent *evening rise* of only one or two degrees comes to be very significant of mischief, if, in health, there ought to be an *evening fall* to that extent. His observations were made by placing Casella's thermometer in the rectum. He says the axilla does not give trustworthy results.—*Glasgow Med. Journ.*, 1869.

In a subsequent memoir in the same journal, Dr. Finlayson investigates the diagnostic value of the thermometer in early tuberculosis. More or less high evening temperatures, with morning remissions, when occurring in the absence of other signs of disease, may often arouse suspicion.

ERRATUM.

On page 308 (April, 1869), in a footnote, third line from bottom of page, for Mr. Buckle, read Dr. Fleetwood Buckle, R.N.

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THE
BRITISH AND FOREIGN
MEDICO-CHIRURGICAL REVIEW.

APRIL, 1870.

Analytical and Critical Reviews.

I.—Medical Tuition.¹

It is as rash for a workman to complain of his wood as of his tools. We shall, on that ground, at once repudiate all sympathy with those who are tempted to shift the blame of the medical student's shortcomings back to his antecedent education. It is for them to take the raw material as it is sent to them, and hew it into the best shape of which it is capable. That raw material is, for the most part, an average youth of about seventeen or so, just able to fulfil the modest demands of the examiners in previous knowledge at the College or Hall, rather shy, easy to lead but hard to drive, willing to learn what is clearly put before him, but averse from thinking, and especially from putting his thoughts into form. What has to be made of him is an observant, thoughtful man, familiar enough with the usual means of relieving sickness to apply them readily. If they expect better material, or do less with it, without just excuse, they are not acting up to the parts which the country assigns to teachers of the art of healing.

There is a variation of species produced by some young men coming straight from school, and some having previously attended the practice of a private practitioner; but what these latter have gained by a certain familiarity with the outside

¹ *General Medical Council. Report of the Committee on Professional Education (1869), with Appendices, containing 124 replies of Medical Teachers addressed by Circular, the 'Reports of the Medical Teachers' Association,' a Sketch of the 'Systems of Medical Education in North Germany, Austria, and France,' and 'Observations on Medical Education.'*

Report of Medical Tutors' Convention, in the 'Transactions of American Medical Association' for 1867.

look of sick people, and the means used to relieve them, they lose by the confusion begotten of tackling complex problems before solving the simpler. The mental condition for teaching purposes differs little.

Some members of the tutorial staffs shrink from the task as put before them, and would raise the standard of common requirements, so as to shut out from entering upon medical studies all who have not already made the best use of their time and talents by drinking at the best sources of general instruction. But it must be remembered that our calling does not offer sufficient rewards of worldly fame or fortune, even to the successful, to attract into its ranks the cleverest and most ambitious; for youths and fathers know full well that forward wits and costly training can be brought to a better market. The attempt, then, to receive only thoroughly educated boys, to raise the standard above the level of other professions, would quickly cut off the supply of students for the wants of the country, and finally drive people to look elsewhere than to us for their needs. An attempt of this sort is the proposal made by the American Medical Tutors' Convention to enforce, as a *sine quâ non*, the knowledge of Greek, against which the Committee of the American Medical Association have very wisely reported. The London Society of Apothecaries are also threatening the same thing, but we trust they will be timely warned.

Equally indefensible with the demand for an unattainably high general education is the suggestion that the pupil, before he be received by his technical teachers, should have such a complete acquaintance with Chemistry and Physics (to which some add Botany and Natural History) as may render it needless for any part of his future hours to be devoted to their acquisition. Now, were the natural sciences allotted a sufficient space in the curriculum of public schools, and even considered essential to an English gentleman, we might, perhaps, listen to this. But, at present, and within all justifiable forecasts of prophetic vision, it is obvious that to ask for such preliminary training is simply to fix the youth's destination to medicine about two years earlier than the already premature age for selecting a profession, or else to delay his diploma for the same period. This is, in fact, to extend the technical training over six years instead of four, and during a third of the time to shift the responsibilities of medical teachers on to other shoulders.

The value of very many of the communications from the teachers at English medical schools is much reduced by this defect. Their scheme is constructed, not as the circular of the Committee of Council requested, "for a four years' course," but for a more extended period; and they require preliminaries

which are not offered to their choice, and more commonly than anything, this preliminary acquaintance with Science.

Whether or not the Natural Sciences would afford a useful engine of general education for boys is as yet an incomplete experiment, the result of which nobody can anticipate. One thing is very clear from the trial, as far as it has gone, that the mere teaching of them by memory is many degrees worse than useless; for it gives the lad a priggish fluency of apparent knowledge, which is a stumbling-block to his acquisition of the real. To teach Science a free use of philosophical instruments, laboratories, chemicals, dissecting-rooms, museums, &c., is essential; and the wear and tear of these is so much more costly than desks and Latin grammars, that one cannot wonder at schoolmasters and parents being backward to send up specimens of a really physically trained youth. The public have no right to ask it, especially as they are not sure to be satisfied with the produce of their request. The freshman must be accepted pretty much as he stands. If schools are improved, he will improve also; but no means can enhance the quality without lessening the quantity, which is already barely sufficient.

Suppose, then, they take the ordinary student as he is sent to them, how shall medical teachers deal best with him for four years? The first and most important step is to exalt the powers of his mental digestion, so that from assimilating words it may come to assimilate things; the pupil has to learn the art of learning by observation. Time is gained in the end by going about this systematically and gradually, by not giving the thoughts complicated problems till they have solved the simpler. Keep, therefore, the junior students sternly out of the hospital wards, till they have mastered Chemistry and Anatomy. And by "mastered" is not meant merely "attended lectures" and "answered questions" on those subjects, but "have shown a manual ability to turn their brain-work into handy-work." Form, and colour, and action, and relation, should begin to come readier into their minds than words. Dr. Attfield is so strongly impressed by this importance of manipulation that he thinks—

"Medical students should learn Chemistry chiefly by means of Practical Chemistry, first performing synthetical and analytical operations at a bench in a laboratory, and afterwards attending a course of lectures—that is, reversing the usual order in which these subjects are studied in medical schools. My reasons," he says, "for this opinion are that of the 500 medical students whom I have instructed in practical chemistry (all having previously attended a six months' course of lectures on chemistry), not 100, on entering the laboratory, knew, or pretended to know, the leading principles

of the science, and three fifths could not even draw out a diagram or equation descriptive of a common chemical reaction. On the other hand, of the 200 or 250 pharmaceutical students who have, under me, worked at practical chemistry for three months (Oct., Nov., and Dec.) of a session, and then attended lectures on leading chemical doctrines, there have not been ten who could not pass a fair examination in the main principles of the science, or give a correct description of most chemical processes. In my opinion, a five or six months' winter course of practical chemistry, consisting of daily lessons of two or three hours each, succeeded by a three months' course of one-hour lectures, given daily, should form the chemical curriculum of a medical student."¹

On the other hand, Dr. Blyth, Dr. Bloxam, and some others, advise that a course of theoretical chemistry should precede practical instruction in the laboratory. Not as a compromise, but because of its intrinsic merits, a preference will be given by most to the plan of Dr. Miller (page 34), who thinks that they might with greatest advantage be carried on "simultaneously." In this he is apparently supported by Dr. Alfred Taylor, who says, "a lecture furnishes a sort of skeleton-map, which the student should fill up by practice in a laboratory," of course as he goes on.

A very similar bouquet of opinions might represent the *consensus* of twenty-six anatomical lecturers, who have replied to the circular of the Committee: all agree in making self-training by dissection the essential point to which all else should lead. One indeed, Dr. Ledwich of Dublin, is so impressed with the futility of lectures alone, that he would away with them altogether. This bold stroke is cited only in evidence of the strength of the feeling among teachers; for in point of fact although such a plan of separate tuition might be successfully carried out by a private tutor, or at a very small school; yet it would be impossible in a large one, for lack of sufficient superintendence; while it is certain that a great deal of the pupil's time may be saved by a judicious lecturer, and an order and zeal infused into his private work, which he might not gain otherwise.

Whether the whole thoughts should be under the command of one of these subjects at a time, or whether they should march hand in hand, has not been discussed by the responding professors. Sir Benjamin Brodie, indeed, for chemistry, thinks that a year at least should be devoted "almost exclusively" to laboratory work; and since he recommends that its study should be "associated with the study of cognate sciences" (page 17), we presume he would fill up the rest of the year with lectures

¹ 'Report of Council,' p. 9.

on physics and the like. But the others generally seem to take for granted that the present plan of studying chemistry and anatomy together should be kept. It is probably better for both the mind and body of an ordinary youth of eighteen, that all his working hours during so long a period should not be occupied with one subject.

There is some difference of opinion as to whether General (that is, textural) Anatomy should be studied before or after Descriptive (or geographical) Anatomy. It might perhaps seem orderly to teach what substances are, before teaching their shapes and where they are, and a chemist certainly would do so. But it is of such vital moment that a pupil should learn to use his naked eyes and hands before he handles a microscope, should think of each thing in its real size before he contemplates its bits in artificial sizes, that the established order is preferable. There is a serious danger of a young man acquiring a microscopic mind, becoming a "minute philosopher," and overlooking the true bearings of actual shape and bulk.

Teachers, in their eagerness to divide labour, and in fear of overlapping and trenching on one another's domains, are apt to draw too fixed a line between Anatomy and Physiology. The result is that anatomical lectures always threaten to fail in interest—for pure description is a dull thing—and of old lecturers were wont to lighten them up with more or less elegant stories and jokes. This is nowadays looked upon as bad taste, so the hour is drearier than ever. Surely it is better to fix attention on the forms of parts by describing their uses at the same time, and not to dread being rebuked by the physiologist for anticipating his duty. If want of time is pleaded, let more and more of pure description be turned over for acquisition in the dissecting-room; the more the better, if it be ascertained by examinations that it *is* really learnt. But let it be always the chief business to gain a knowledge of form, while the illustration of uses is given to assist that. The professor of chemistry might add an interest to his latter lectures in the same manner by interpolating a little physiology.

A very considerable quantity of unsystematised physiological knowledge will be picked up in this way, so that a much shorter systematic course may be given than would otherwise be required. Its business should be to put the unsystematised knowledge in order in the pupil's mind, to show as many experimental demonstrations of it as possible, and to fill up certain blank subjects which neither the chemist nor the anatomist can have alluded to. But with all that, physiology cannot be made an engine for infusing powers of independent observation and self-instruction in a degree at all equal to anatomy and chemistry.

Some of the correspondents of the Committee commend courses of "Practical Physiology," and would enforce attendance at a physiological laboratory. But how many physiological experiments, worth anything except for show, do they think a pupil could get through in the few months that can be spared for such continuous daily labour? Time spent in a physiological laboratory to profit must be measured by years, not by months; and if there were the time to give, such a devotion of it might turn out a Müller or a Bernard, or a Brown-Séguard, or it might not, but certainly not a good country doctor. It is certain that lectures must be almost the whole machinery for teaching physiology in the school. "In the school," because here first the student may be advantageously assisted by the library. Some few at least of the methods of investigations on which the science is built up should be gone right through, and others, which depend on series of figures, should be read at leisure, so that the mind may appreciate what the evidence of physiology really is. Here judicious selection and direction are imperative, and should be given publicly in the theatre. Conversations on these books and informal catechising should follow.

At this stage of the studies the first examination for diploma should take place. It is a definitely marked period of the pupil's mental development which should then be tested, and further progress stayed till he has got over this. He should now be able to exhibit the manual and mental ability of proving to himself and others the reason of the faith that is in him, in respect of the form and properties and composition of the human body, its actions on external matter, and the action of external matter on it—that is to say, Physics, Chemistry, and Anatomy, culminating in Physiology.

These are the subjects included in the Committee of Council's list, under the head of—

1. Physics.
2. Chemistry.
3. Anatomy.
4. General Anatomy.
5. Physiology.

(We have altered a little the order in the 'Report,' page xxii.)

Let not these matters be called subsidiary, or preliminary, or introductory, to medical education. They *are* medical education. Does one reckon a foundation or a basement of less moment than a coping or a capital? The only folk known to have done so have suffered for it. The pupil has been learning to be observant of those things with which his whole soul has to converse for his whole life. Till he has shown that he has

learnt it, let him proceed no farther. Nothing can make him a healer.

At this examination, above all others, let the examiner be patient, and broad-hearted, and watchful, or some of the pupils of best promise will be rejected. The candidate should be conscious of having before him a friend who is trying to find out what he *can* do, and not what he *cannot*. Let him not be made to feel afraid of using the wise phrase "I don't know." Let him be confident that some other inquiry will give him an opportunity of showing what he is. He is probably a slow thinker—many of the most useful members of society are so at his age—so let him be tested by the operations of his hands more than by the glibness of his tongue. Depend upon it, fluency in youth shows a ready memory of words, rather than fulness of information. Good examinations cannot be rattled over like a lesson; they must necessarily be leisurely affairs, and probably the principal cause of their remaining imperfections is their being conducted in too great a hurry.

The pen refuses to be withheld from repeating a conviction of the importance of this first examination being of a searching kind. It will do a candidate no harm to be sent back once to his studies for such times as the case requires. This need stamp no stigma on an undeveloped mind, which, in fact, simply wants an application longer than the average to the training process. But after the second pluck a young man should be made to understand that he has mistaken his vocation. Of the fact one may be quite sure; and he had better leave the vain pursuit before he costs more labour and disappointment to himself and others. If a medical student cannot by dint of chemistry, anatomy, and physiology, be led to draw his ideas directly from nature, instead of from the words of others, he never can become the observant thoughtful man, who alone is useful in our profession. It is said sometimes that a mere poll-parrot may still be serviceable as a routiner, and that all our young doctors need not be original discoverers and creators. Yes, but they need; unless a man be in every case of sickness a discoverer to a certain extent, and a creator to a certain extent, he can heal only by haphazard, and on the doctrine of chances he will delay rather than hasten cure. The difference between John Hunter and the practitioner who does the minimum of genuine good, is a difference only of degree; what is now decried as worse than useless is another order of mind altogether, in which memory takes the place of observation.

Up to this point the whole, or almost the whole, object of education should be what the word indicates, a *drawing out* of latent qualities, not as some introductory lectures paint it, a

“laying up stores of useful knowledge.” But though its main intention must still remain the same, the warehousing process may now be a legitimate part of it. The subjects next to be studied consist in an application of the powers already acquired to the facts of actual duties. They are—

1. Medical Chemistry.
2. Pathological Anatomy.
3. Therapeutics.
4. Pharmacy.
5. Medicine.
6. Surgery.
7. Midwifery.
8. Hygiene.
9. Forensic Medicine.

These are the studies on the Committee of Council's list not yet provided for.¹ A formidable list to look at ; but it should be explained that some of the nomenclature partakes of a wind-bag character, and that the names of the subjects do not imply the same as when used by a librarian for classifying books, or by an author on his title-page. Take “Medical Chemistry” for example ; an university professor or an author would embrace the greater part of physiology and organic analysis ; but really the Council's requirements would be fulfilled by a young man being able to test secretions and detect their differences in the microscope. A simple thing enough, if not too much talked about ; and best capable of being taught by two or three desks in the laboratory being kept for hospital students, who should be encouraged to bring in from the wards matters for analysis under the teacher's eye. Half a dozen half-hours will produce an able manipulator.

Pathological Anatomy again only means the knowing by sight some forty or fifty post-mortem appearances at most, and the being able to distinguish by the microscope a few of their textural peculiarities. Pupils are rather fond of the dead-house, and if instead of letting them stare about, or only write notes of what he says, the prosector were to take the trouble of setting to work with their scalpels several of the bystanders on each occasion, the greatest part of pathological anatomy might be communicated. Half a dozen peripatetic lectures in the museum each winter, to those already able to make an autopsy, might complete the business in the best style.

Therapeutics, as a separate study, is a novel requirement, and not a good one. There is nothing referable to this head which cannot be taught as part of *materia medica*, medicine, surgery,

¹ ‘Report of Council,’ p. xxii.

or hygiene. To dissociate the thought about drugs from their uses, diseases from the mode of healing them, is most pernicious, and nothing tends to do so more than the delivery of lectures by different professors at different times. This may be a weakness, but it is a human weakness. Therapeutics and *materia medica* should be taught together, as of old.

Pharmacy proper, that is the art of making up medicines, is best taught in a shop. But many have strong doubts whether it requires to be taught at all. One can hardly imagine a young man, who has worked in a chemical laboratory and passed a practical examination in chemical manipulation, being so helpless as not to be able to follow the directions of the *British Pharmacopœia* and make up his own medicines. If he wants to become an assistant, he will soon find out how to make up his master's also. If he wants to deal in "physicians' prescriptions accurately prepared," he must learn the art in a shop, as was said at first.

Materia Medica is a different subject, consisting of that personal acquaintance with his tools which every workman ought to have. It seems peculiarly capable of being taught to a class by demonstrative lectures. While the audience is handling, smelling, tasting, and otherwise becoming familiar with the articles of the *Pharmacopœia*, instruments, bandages, &c., the professor can be telling their history and uses. The best classification seems that founded on natural history, as thus the mere order of lectures inculcates a considerable quantity of knowledge. Those who provide the specimens should not grumble at their being pulled to pieces. This course may be made very interesting and even amusing without loss of time.

There enters now on the scene a most serious trilogy of subjects, Nos. 5, 6, and 7, *Medicine, Surgery, and Midwifery*. They are only exceptional students who do not intend to be physicians, surgeons, and accoucheurs, all at once and equally. Those who wish to qualify themselves as pure practitioners of the several branches of the profession do not need much aid from teachers, and at all events are not the subject of the present pages. But even they would be the better for having studied all three subjects up to the minimum degree necessary to pass an examination. For if surgery be held to include all maladies admitted into surgical wards, and midwifery the diseases peculiar to mothers and children (as the responding lecturers seem to agree), pure physicians will certainly take charge of such; and the strictest of surgeons or of accoucheurs can often not escape acting as a physician.

Like the studies with which we fully occupied the time of the pupil up to the first examination, medicine, surgery, and

midwifery are not merely a collecting of useful knowledge, but a true training process ; indeed they are the final and culminating training process for the future practitioner. They should be made his prime business, to which everything else must yield, if he be so pressed for time that any yielding is necessary.

As to the means of instruction, all teachers seem agreed that there should be a combination of lectures and clinical work in these branches. But they seem not sufficiently to have considered the advantages which would arise from a more intimate inosculation of the branches among themselves. Perhaps the being addressed by circular separately on their own department only has withdrawn their attention from the matter. But we are sure that a physician, a surgeon, and an accoucheur, acting independently on a pupil, will take much longer and produce a much worse practitioner than if they were to work upon him in harmony. To take first the matter of the lectures ; here is a physician who, not only as a part of general pathology, but again *apropos* of abscess of the liver, &c., holds it necessary to explain the whole doctrine of pyæmia ; pyæmia again, possibly in the same week, is enlarged upon by the surgeon as a consequence of wounds ; and the accoucheur again goes over the same ground when he arrives at puerperal fevers. "Inflammation" has been known to occupy several lectures, in both the medical and the surgical course ; and "cancer" has been treated of, and demonstrated with specimens by all three within a month. Measles, scarlatina, &c., are always fully given among general diseases, and among diseases of children also. Indeed, several pages might be filled with familiar instances of repetition. Bad economy of time this for a poor student who has only four years to grow from a schoolboy into a doctor. If the various lecturers have "views," it is confusing indeed, but not so bad as wasting time by repetition. A concordat between the professors is necessary at the beginning of each session, to agree what subjects each will take in hand ; or lecturers will continue, as now, to dissipate energy in overlapping. "At the beginning of each session," because, in many cases, it will be for the good of the school, and oftener still tend to preserve peace among colleagues, that they should take it in turns to handle certain topics ; and also because, as science advances, a frequent rearrangement tends to condensation. Such a meeting would also enable professors to exercise more faith that a knowledge of physiology, necessary to profitable hearing of their lectures, had been communicated. We should not have Dr. Beale complaining of students coming into the medical wards ignorant of the anatomy of the heart, and Dr. MacIntock, Dr. Graily

Hewitt, and Dr. J. G. Wilson, classing the physiology of generation and embryology as a part of midwifery lectures. (See 'Report,' page 135, and "Midwifery.") If the students were kept to their physiology and chemistry till after their first examination, these scandals would be unknown.

Under the head of Systematic Medicine ought to be included all of the science of Hygiene suitable for being taught by lectures. To separate in the hearer's mind healing and health is dangerous and destructive of true progress. It has a tendency to divide our successors into two parties, the druggers and the sceptics, a whirlpool and a rock between which it is very difficult to steer them in safety. It is true there are a variety of important matters relating to the influences of air, exercise, food, clothing, &c., which would unduly swell the course; but to appreciate these to any advantage, not only statistics, but a mass of recorded detail, is indispensable, which renders their study from monographs easiest and most convincing. The lecturer should confine himself to principles which can be very shortly enunciated to those who have been taught physiology enough to pass a real examination. Moreover, all medical chemistry which is not purely manipulative should be interfused in the pathology and therapeutics, which we combine under the name of systematic medicine. Insanity, also, should be treated of along with other diseases, and not made a separate study as at some schools, as if the ordinary students were never likely to see a mad person, or as if Psychology were such a coy nymph as to tell her tale only to those who surrender themselves wholly to her charms!

Besides the bad effect on the mind of the pupil, teaching him to see differences readier than to see resemblances, the subdivision of subjects has the further fault of multiplying the lectures out of which he has to pick the knowledge he requires. Like cut-off portions of a *Hydra viridis*, each new section swells and grows and pullulates till ready to be itself divided. Each zealous young professor magnifies his office, demanding more and more of the pupil's attention, and illustrating the wide extent of the study. And then when he is asked to account for the ignorance of his speciality displayed by candidates, he answers that the time allotted to the subject is much too restricted. That is not the truth; the real reason is the dislocation of the subject from allied materials of thought in the general course of instruction, which mutually assist one another to become rightly impressed on the mind, if thus rightly taught.

The courses of systematic medicine, surgery, and midwifery, should discourse of elementary principles only. One or two of the responding teachers have urged, as an objection to thus

shortening them, that they would be rendered dull and bare. This need not be so, for young men are even easier attracted to general pathology than to special, if its connection with their previous physiological studies is kept up, and its hygienic bearing on the health and happiness of all around them is duly put forward. More frequently, too, in general pathology than in special can a pat illustration be found ready at hand in the wards or the mortuary; for broad, much-embracing agencies are being described; the aim is to group rather than to differentiate, and therefore the commoner the case the better.

The great advantage of shortening the systematic courses would be to relegate a larger share of teaching to the clinical department; a most desirable result, for the knowledge thus gained is more real, more handy, and more ready to beget action, than what is gained elsewhere. And we would fain force the student into drinking from this source, by not supplying his needs otherwise. If he knew that he would be examined on certain points, say, concerning pneumonia, or dislocations, or childbirth, which were not taught except clinically, he would bestir himself thus to learn them. Not only is this knowledge more valuable, but there is also greater facility for the teacher's seeing that the pupil really gets it. A zealous lecturer may exhaust himself in efforts to make a subject interesting, but he never can tell at the time whether the calm-faced class is drinking in wisdom or asleep with the eyes open. The aspect of the British youth is very similar under both circumstances. But in the wards, unless he is visibly doing something, he may justly be upbraided with idling.

Clinical lectures have many faces. They are often made use of as a mere opportunity for a gentleman with views, or a set of pet subjects, to dilate thereon, to bring forward original work, to construct ornamental harangues illustrated with his private experience, so as to give a lively turn to the matter, which afterwards makes a goodly show in the pages of a periodical. No wonder that students prefer to read this in the library, and that indignant teachers are driven wild enough to pronounce all lectures waste of time (see Dr. Ledwich's reply to the 'Report'). But there are real clinical lectures, and than these we cannot imagine anything more profitable. It is possible to take the pupil from the wards straight into the lecture-room, and there carry on the demonstration of actual disease and its cure, which has been begun at the patient's bedside. There is every degree of transition between these two sorts of clinical lecture, and each is valuable as it recedes from the first and nears the last. A clinical lecture of the right sort, or as nigh the right sort as can be, should follow every visit; and to make that convenient

to all parties, let the clinical lecture-room be quite close to the wards.

It is to the work in the wards that the attention of the teacher most needs to be directed. There is at most hospitals an amount of means of instruction running to waste which will lie heavy upon the wasters at a day of reckoning. If you visit the beds, you will find many inmates who have never had a medical student speak to them, much more take their cases. Nevertheless there is not one of these but may have been made profitable for doctrine, for reproof, for instruction, or all three.

The first and most essential detail of organization is duly to divide the pupils among the teachers. A clinical class should never exceed the number which can stand round a bed. Ten is the extreme which British breadth of beam will permit. That is, perhaps, a better way of putting it than the proposal of Dr. Chambers ('Report,' page 138) to adopt "the classical rule for a supper party, a number between that of the Graces and of the Muses." The description, by the Dean of St. Mary's, of the way in which this is attained at that school, shows how feasible and business-like the proposal is ('Report,' page 238).

The first business of the clinical instructor should be to put the pupil in the way of using his senses for the detection of morbid states. The aberrations from the normal of secretions, sounds, colours, &c., should be demonstrated by test-tube, microscope, stethoscope, measure-tape, colour-scale, sphygmometer, and every new instrument of precision.

There is a vulgar prejudice respecting the use of these comparatively modern aids to diagnosis. The more accurate and scientific they are, the more the ordinary student is allowed to respect them as meat for his master. He is led to think of them as unadapted for the rough work of life, and helpful only to those who can afford time for minute philosophy. Now this is to take a completely upside-down view of the case. The master is just the person who can easiest dispense with these contrivances, and the student the person who most wants them.

Could one of our contemporary necromancers call up the spirit of Sydenham, he would probably, with the British Pharmacopœia before him, rap out as good a prescription for a particular case as the last elected hospital physician, with all his tests, his 'meters, and his 'scopes. It is not for him whose "old experience doth attain to something of prophetic strain" that mechanical appliances are required, but for the junior who is straining every nerve to rival the object of his just admiration. Machinery levels up. Moreover, its use is very easy to acquire, when it is the teacher's interest to communicate it in as short a time as possible. Watch two clinical clerks taking a case, and

the senior inducting the junior into some of the "dodges" of auscultation. Does he lecture to him? Not a bit of it; but he listens himself, imitates the noise with his mouth, makes his friend listen, and with a very few words after a very few cases, the one is as adroit as the other. An excellent expedient with a class is having them up periodically to a sort of stethoscopic drill, in which the whole of the chest is gone over in a regular order, and notes made of observations to be subsequently corrected. Instruction in the ophthalmoscope, laryngoscope, sphygmograph, requires each pupil to be taken separately, but only for a few minutes at a time; and the less lecturing there is the better, for the pupil will feel all the more ashamed of not picking up so simple a piece of knowledge—that is, provided always that he finds *a tutor ready at hand to communicate it*. The same proviso will apply to instruction in bandaging, plastering, poulticing, catheterism, and half a dozen other surgical devices which can be given easily by demonstration, and very hardly by words. There was a phrase put in italics just now, because that is the point at which the shoe pinches. Let no one expect, since experience shows he cannot get, tutorial work out of the senior members of an hospital staff; if, besides curing the sick, they attend to the recording of cases by the clinical clerks, and make a few practical remarks after the visit on the patients visited, they will have done a fair public day's labour. The routine of grounding the class in elementary clinical knowledge would distract them from their proper occupation. Moreover, it may be questioned if they would do it so well as younger men. Long experience has taught the English universities that the best instructors for minds *in statu pupilari* are those who have the most recently received instruction themselves. There should be employed in the work of teaching, much more than is as yet done even at the activest schools, the medical officers of the out-patients. But besides this there should be some real assistant physicians and surgeons, that is to say assistants to the physicians and surgeons—young men who would utilize for educational purposes the patients, by the permission of their chief, and in harmony with his more discursive teaching. At the larger schools, with a full class to each, they ought to have a share of the profits, but at the smaller probably they would have to be content with the honour and the precision of mind which they would acquire.

Forensic *Medicine* (No. 9) is an unhappy misnomer; it has nothing to do with the *ars medendi* at all. A healer is not a good detective, and the evidence which he is trained to value is not the evidence suitable for a court of justice. To avoid being snubbed by the judge, puzzled by the counsel, and misunder-

stood by the jury, the ordinary practitioner is happiest if able to say that he has never been taught forensic medicine, and can depose only to matters of fact as an unskilled witness. Were the truth generally professed, if our brethren would be satisfied not to know what it is not their business to know, lawyers would soon see the necessity for having superior men specially trained to this most important branch of jurisprudence, to act not as witnesses, but as assessors to the legal authorities. Seeing the importance of the subject, Sir Thomas Watson has wisely gone beyond the strict letter of the questions set him in the circular, to give his opinion against lectures on forensic medicine forming part of the regular curriculum (page 160), and to urge its being made a separate speciality.

As to the system of learning by lectures, as a whole, the bearings of the question have seldom been better stated than in a conversational sort of address given at St. Mary's Hospital by the Archbishop of York, speaking not as an archbishop, but as an experienced Oxford tutor :

“I do think, on the whole, for a place where subjects are to be taught to the average run of men, where you consider the man more than the subject, the tutorial system, in some of its modifications, is the system most profitable to be employed. I have heard from your Dean that the number of lectures given to the students is extremely great. I forget how many in the week the student is expected to attend.

“The Dean : From fourteen to twenty-one.

“The Archbishop : From fourteen to twenty-one in the week ! That is something exceedingly great. At Oxford we consider twelve in the week a very good average ; and at the end of a student's course, he will often come to the tutor and say, ‘I am pursuing my own studies, reading this and that book, and have attended lectures on this and that subject before ; could you not let me off the lectures which call me out of my room and interrupt my course of study considerably ?—could you not trust me a little by myself during my last term ?’ It is a peculiarity of the system that when a man is known to be at work, and comes to his tutor and says, ‘This is my last term. I am going up for examination next term, and I wish to be excused some of the lectures,’ his request is granted as a matter of course. You will see that there is a very great difference between your system and ours with respect to the number of lectures. I confess my ignorance of the whole matter as regards you ; but still, having formed my own impressions of human nature, I am disposed to say a word in favour of *mercy* with regard to the number of lectures. I said just now that it seemed to me almost impossible that a man could sit three or four hours constantly receiving and giving nothing back without wasting a considerable portion of that time. I believe that the habit of attending lectures is not a thing given by nature. It must be acquired ; and I do not know that we

are always willing to give much trouble to acquire it. The consequence is that a good deal of information which is poured in must bubble out again, and be entirely lost. It seems to me quite possible that a hint from our system may profitably be taken by an excellent institution such as this is. Be sure that the minds of the young men are in active operation; and then, when you know your men,—when the tutor can see that they are doing their work in his section or class,—let him trust them a little more, and not exact so many lectures. * * * * * If I am not mistaken, there are even stronger reasons, in regard to the studies pursued here, for leaving the students somewhat to themselves, than with regard to us at Oxford. Our business is with books, and when a man is summoned to lectures, he can put a mark in the book he is studying, come back, and take up the subject where he left off. Nothing is changed after his lecture his over. But I take it, though I know but little of chemistry, that in the case of chemistry you cannot leave your test tubes and retorts simmering and fizzing, go off to lectures and return, without loss of time and material, and without a certain fret of one's thoughts. I presume the same is the case with regard to dissection, and to a certain extent, with regard to everything requiring the use of hands and instruments. If you can trust your pupil, and be sure that he is at work, better let him do it, and not tease him to come backwards and forwards to be instructed."

Let it be hoped that one of the first acts of medical reform accomplished, by either the present or a future Council, will be to free the schools from the obligation of dragging their pupils through such a wearisome hammering by continuous lectures as "schedules" now enforce. The slavery is demoralising to both tutors and class. In the meanwhile, till this wicked discipline can be removed, which is much to be wished, we commend to the notice of those junior lecturers, who are not too old to change their plans, a proposition made by several, that the hour spent in the theatre should be filled up by the explanation and examination on some text-book of the matter in hand. In anatomy and chemistry and physiology this is most feasible, and Dr. Richardson (of Dublin), Dr. Chambers, Dr. Bence Jones, and Dr. Shann, express their opinion that it might be done even in teaching the principles of medicine (see 'Report,' pp. 76, 138, 147 and 156). The difficulty lies in the want of a text-book free from "views."

As to the portion of the four years which ought to be allotted to the several subjects, that is primarily governed by the date at which the first examination takes place. Those teachers who have expressed a definite opinion on this matter would have it not earlier than two years from entry on professional education; and in this we are disposed to coincide, though the Committee of Council propose eighteen months as time enough

to give to preparation for it. If any time at all be made compulsory, it should be the longer; but the matter is better left to the judgment of the individual schools; some might be long-time schools, and some short-time schools; and the lessons they would get as to the advantages of either plan from a strict examining board would soon lead to the knowledge of which was best.

A uniform practical examination at the end of the four years is essential to all real improvement.

Allowing the pupil two vacations of six weeks each, he has forty weeks or 240 working days in the year. For five weeks before going up to either examination it will be prudent to exempt him from attendance at all formal lectures, and it will be necessary to accommodate the whole class to this exemption. But for the rest, we should be disposed to administer one lecture on the average per diem, in all 900 lectures; and of these the following suggestive division may show the feasibility of the scheme:

First and second years	Chemistry	.	.	.	180	
	Anatomy	.	.	.	180	
	Physiology	.	.	.	180	540
						<hr/>
Third and fourth years	Materia Medica	.	.	.	60	
	Medicine	.	.	.	100	
	Surgery	.	.	.	100	
	Midwifery	.	.	.	100	360
						<hr/>
						900 lectures.

Each set of lectures should be divided into two courses of a year's length; the first course for freshmen, the second for seniors. It is vain to attend the same course twice; when a young man knows he is going to have a thing repeated, he postpones thinking about it till the last.

It is a bad custom to divide the chair of a systematic subject between two lecturers. It wastes time in repetition, it encourages loquacity, and lets responsibility fall. Should the two differ, or appear to differ, their mutual corrections beget scepticism in the hearer. He feels like the old verger at the university church, who, after hearing for forty years the excellent preacher in the afternoon contradicting the learned divine in the morning, thought it a special providence that he remained a Christian.

We would enforce, really enforce, employment about practical work for four hours at least daily. During the first two winters and summers that time may be divided between the laboratory and dissecting-room; during the last two the pupil should be

brought into personal contact with patients. And inasmuch, according to the scheme above given, he would have during his final two years nearly 200 more hours free from lectures than in the first two, he might give those up to the learning of sundry useful matters, the order of which is of no account, since they are, less than others, training occupations. Reference is intended to pharmacy, or making up medicines, vaccination, operations on the dead body, bandaging on a dummy, going through the drill of midwifery on a phantom, extracting artificial teeth, rigging up slings, ambulances, and beds, constructing temporary splints, &c.¹ Pharmacy may be learnt in the hospital shop, and for the rest there ought in every school to be a room properly fitted up, out of the sight and hearing of the patients. These are conventionally called "minor operations;" but it must be remembered that they are *minor* only so long as you do not neglect them; under the latter treatment they become most terribly *major*. There are ten times as many losses of health and life from bad bandaging, foolish slashing, awkward catheterism, and general bungling about what every house-surgeon can do, as there are from ignorance of how to cut off legs and tie aneurisms of the iliac. There is something very invidious about the distinction, and it is not at all justified by results.

The annual five weeks which it would be wise to keep open for the final polishing up of the candidates who are about to submit themselves to examination, must, of course, be made general to the classes, and we should like to see them employed in private class examinations. This valuable means of education has been too much neglected, seemingly from lack of a time to give to it. The student would thus have to go through four examinations—two public and two private, and would, by the fourth, have become sufficiently used to the process not to be plucked out of shyness or panic.

The distribution among surgeons, physicians, and accoucheurs, of the pupil's four hours daily of actual contact with patients, cannot be laid down according to a fixed rota; for to send every freshman to begin at certain wards would overfill them, and leave others empty. But we contend for equality. We hold that accoucheurs, physicians, and surgeons, ought to bestow an equal portion of time upon the communication of the practical part of their branches. Medicine and surgery, dealing more generally with pathological principles, had better entirely occupy

¹ Shall we be liable to advertisement duty if we say—"Wanted, a good artificial urethra, fitted with enlargeable prostate, strictures, &c."? Such a piece of machinery would spare patients much pain. The fines on false passages would provide for frequent renewal.

the third year of pupilage, and be studied in the marked typical cases, repaying daily visits to the wards. During the fourth year we would employ the pupil for two or three months as a clinical clerk or dresser, which would induct him into a certain feeling of responsibility; but the greater portion of his time should be spent in frequenting the uterine wards, and in seeing the diseases of mothers and children among the general out-patients, who are all available for this purpose. Certain hours, also, should be appropriated to the cutaneous and ophthalmic departments of the hospital. The hours of work should be carefully laid down for each student, to avoid that pernicious dawdling which is the special vice of young men in the present day. Attendance on a sufficient number, say twenty, women in child-bed, had better be made, if possible, a vacation task, as it is a grievous interruption to regular studies.

One reason why students are uninstructed in midwifery is the difficulty of securing the punctual attendance at the school of doctors practising that branch. All of us have known even lectures omitted, and a physician called away before he had been half round his wards. This is a fraud, and it is usually done under the shelter of an honoured name. Regular returns should be made to a central body of the unpunctuality of teachers, and the punishment of cashiering freely exercised on stiffnecked sinners. An annoying part of the matter is that the valuable time of which the pupils are robbed is, often as not, frittered away at the normal childbeds of fidgety rich women. It would add much to the dignity of the obstetrical art if some of its distinguished exercisers would be consultants only, and not throw away their energies in doing what a midwife can do as well. An attorney-general would not prosecute an extortionate cabman, even at the bidding of a duke.

It has been objected to the proposal for making the training of the general medical student tutorial in character, that we should have no more of the brilliant original lectures by distinguished men which decorate our literature. Indeed, but we think there would be more scope for them. There would still remain the superior students, six-years men, and young practitioners. The establishment of professorships for their guidance and encouragement, where the lectures were not diluted by the elementary matter now necessary, would give an opportunity such as is very scarce under the present régime.

Would you bind down to an unbending rule all medical schools? By no means; leave them the greatest possible liberty as to the mode of instructing pupils, but be most strict in seeing that they acted up to their profession in possessing the means for doing so. What we have sketched out is merely a demonstra-

tion of what is possible, devised, not, indeed, for what Aristophanes calls Nephelo-cocky-guys—Cloud-cuckoo-towns, founded on misty imaginings, and peopled with mocking reiterations—but for slight modifications of existing schools. We would have them visited by a central authority, and licensed only if furnished with proper museums, dissecting-rooms, laboratories, and theatres; but, above all, with a *competent staff of young clinical tutors*, who could enforce that practical instruction on which we have laid so much stress.

The strong moving power of all improvement of average education must be the authorised examinations for diploma. They must be stricter, more practical, and, above all, more leisurely. For the convenience of school discipline we would hold them annually only, but that annual season should occupy several weeks. They should be held in broad daylight, as if they were a real day's business, and not allowed to be huddled over by candlelight, as if it were some mediæval formality not worth the cost of working hours. It would obviate many objections if they were open to the public, like the medical examinations at Oxford.

Six sevenths of our pupil's time is provided for. Is it right that the remainder should be a mere blank, with which teachers have no concern? Is it nothing to those, who occupy his mind with visible things through the week, what views of invisible things he acquires on Sunday? If he goes home, of course domestic influence should be paramount to all that other guardians may do. And if he belongs to a communion conscientiously averse to the dominant Prayer-book, that conscientiousness will probably secure religious supervision. But there is a crowd of homeless students, by no means viciously inclined, who spend the day in complete listlessness. Sometimes dropping into church, oftener not, and if there hearing nothing specially suited for them, hanging about the hospital from mere want of something to do, or lying on the sofa smoking and chatting. The majority look on themselves as virtuous; and so they are, compared with a small minority who make a decidedly worse use of their holiday; and they do not come in contact with a still smaller minority who employ it better. Why should not hospital chaplains consider these wanderers as part of their flock, and have special services adapted to their needs? On Sunday the patients in the wards want their pastor less than on other days, and some of his time might be well spared to the work now proposed. United worship would soon lead to united interests and united self-improvement, such as cannot be gained by listening to sermons; for the means would here be specially adapted to the persons and circumstances. A clergy-

man could hardly fail to be interested in reading Butler and Paley and Argyll with students of nature: it would be a happy task to supply the missing link between Darwin or Huxley and the Bible; to show how "natural selection" implies at every step a supernatural Selector, how the "reign of law" in physics is absurd without a Law-giver; he would trace the thousand points of analogy between God's character in revelation and in the material world of to-day; he would lead the pupils to see the kinship there is between the highest physical science and the highest spiritual; and he would thus guard them against superficial teachers like Dean Close, Mr. Bradlaugh, or Pius IX, who are fain to persuade their followers that the two sorts of knowledge are antagonistic. It is unfortunate that a working model of such a chaplain is not to be pointed out at present, but former King's College men still cherish a grateful memory of the wise influence of Frederick Maurice. In this matter, as in many others hinted at in these pages, "peccavimus! peccavimus!"

II.—Ringer's *Handbook of Therapeutics*.¹

DR. RINGER'S treatise on therapeutics came too late for our article on works of that class; but this is, perhaps, the less to be regretted, inasmuch as it amply deserves a separate notice. We have hitherto had no book resembling the one produced by Dr. Ringer, for the majority of our works included *materia medica*, as well as therapeutics, and the mode of classification adopted has been either alphabetical, according to natural history,^a or abstractly therapeutical. Dr. Ringer has, in his present work, adopted almost exactly the classification of Buchheim, one which we have already had occasion provisionally to recommend; and the pages of Buchheim's work, as well as the more conventional and, perhaps, more elaborate, treatise of Clarus have yielded of their stores to our author. Dr. Ringer begins boldly; he plunges *in medias res*, without a word of introduction; and, considering the nonsense we sometimes encounter in such introductions, the intimate blending of fact with fiction, inference with hypothesis, we are inclined to think he has done well to avoid even the risk (Dr. Ringer is

¹ *A Handbook of Therapeutics*. By SIDNEY RINGER, M.D., Professor of Therapeutics in University College, Physician to University College Hospital. London. Pp. 485.

far too clear-headed to perpetrate such a *mélange*) of stating as facts doctrines capable of being disproved.

The first article deals with oxygen, which the author says is rarely used in medicine ; it certainly is, in some form or other, a good deal employed in irregular practice, although more rarely by ordinary medical men. When so employed, it is generally in the form of ozonic ether or peroxide of hydrogen, to neither of which does Dr. Ringer here allude, although peroxide of hydrogen is mentioned further on. Both of these substances—if, indeed, as ordinarily used, they can be spoken of as different—would *à priori* be more likely to be useful than simple oxygen ; and such has been asserted of them, especially with reference to one disease here mentioned as improved by oxygen, viz. diabetes. Long ago Beddoes fancied that in oxygen he had discovered a panacea, but experience has not been in its favour. Dr. Ringer does not say how oxygen is to be administered ; we presume, by inhalation. There does exist, however, an Oxygenated Water Company, whatever their product may be.

The next four paragraphs are badly conceived ; they are respectively entitled “The Internal Use of Water,” “Cold,” “Ice,” “Warm and Hot Baths.” We must deal with these collectively, for the matter is confusedly arranged. Thus, the first sentence tells us how hot water will sometimes relieve toothache ; then, at another time, cold water will do the same. Now, it is obvious that in this case it is the heat or the cold, not the internal use of the water, which is the thing to be considered. We do not grumble at the matter provided for us ; it is the way it is served up we complain of. Perhaps the portion immediately succeeding those to which we refer would have been better headed, to use an Americanism, “Drinks,” for on this subject our author gives what, we are bound to say, is good sound information, but, as before, not clearly arranged. Certain statements, however, we must negative ; one especially, to the effect that excess of water drunk is mostly thrown off by the kidneys ; the fact being that this depends on the temperature and work of the body. Take a reaper in harvest ; the most of the water he drinks is thrown off by the lungs and skin, only a small proportion by the kidneys, and so in other instances. Under “Cold” the effects of cold sponging, bathing, &c., are chiefly considered. When dealing with the tonic effects of cold, Dr. Ringer seems to teach that, under its influence, rapid change takes place in all albuminoid tissues : this is more than doubtful ; but further he makes no reference to fat ; no doubt, change in the one implies change in the other, but in keeping up the bodily temperature fat is the more important agent. The remarks on sea-bathing are good, and worthy of all attention, as are those on other kinds of baths.

The article on ice is also fairly good ; the author strongly recommends its use in diseases of the throat ; in this we quite agree. He also alludes to the use of pounded ice and salt as an anæsthetic ; this is not much used now since ether spray was introduced ; but as it still may be so when the more scientific application is unattainable, he might have added a caution against its prolonged use, which, as frost-bite, may end in gangrene of the part. Such a result is not unknown in the annals of surgery. Warm baths are not treated at the same length as cold baths ; and the hot-air and vapour bath, both invaluable auxiliaries in the treatment of certain diseases, are still more curtly dismissed.

With charcoal we enter on the domain of ordinary *materia medica* ; and here we are again glad to endorse Dr. Ringer's views as to the frequent uselessness of charcoal poultices as deodorizers of unhealthy sores, and the advisability of preferring a layer of charcoal in bags over the usual poultice. Speaking of carbonic acid gas, Dr. Ringer overlooks one use to which it is put with advantage ; that is, as an inhalation to allay cough dependent on bronchial irritation ; it is wonderful how well this plan sometimes succeeds. The article on nitrous oxide which follows is by Mr. Clover, a well-known authority on anæsthetics, and a very good one it is. The view of its preventing oxidation is assumed as the clue to its physiological action. Nevertheless, it does not prevent the expiration of carbonic acid gas, as may readily be proved. Under the heading of sulphur, Dr. Ringer describes some of the methods of using it against itch. Thus, Hardy removes the superficial cuticle by alkalies or their carbonates, and then applies a sulphur ointment. But if there be eczema or impetigo, Hebra's plan is better ; he makes use of an ointment composed of chalk, sulphur, tar, common soap, and lard. Sulphur is also given internally as a laxative ; this effect Dr. Ringer thinks depends on its conversion into a sulphide by the action of the bile. The sulphides (of potassium, sodium, ammonium, and calcium) constitute the next group. Their action is somewhat similar to that of sulphur, and they are used for somewhat similar purposes—for instance, a lotion formed by boiling lime with sulphur is recommended as a powerful remedy for the itch ; the same may be said of the sulphide of potassium. Whenever the patient could bear it, we would strongly recommend soft soap as a basis for these, so as to form a kind of ointment, which may be *rubbed in*. The lotion cannot so well be employed. Dr. Ringer would seem to mix up with the sulphides their product, sulphuretted hydrogen, for he suddenly begins to speak of “ this gas ” and its effects, and otherwise confounds the one with the other. The author strongly recom-

mends the sulphides in the scrofulous abscesses of children, which are often so troublesome, and where, he says, the sulphides do good when steel and cod-liver oil have failed. The dose prescribed is certainly not a large one, being a grain of sulphide of calcium to half a pint of water; a teaspoonful to be given every hour. The solution must be freshly prepared.

Chlorine and chlorine-yielding substances are next discussed; the author has nothing very new to say, except that permanganate of potass does not stimulate a sore to which it is applied. To this we decidedly demur. If used in very dilute solution, its influence is not so marked; but if used half pharmacopœial liquor strength, a convenient form, its influence is most marked. In this strength it has been used with great success in the treatment of gonorrhœa.

The next two chapters are of considerable importance; they relate to iodine and iodide of potassium. The former chapter relates mostly to the local effects of the remedy, the second to its constitutional influence.

Now here we are brought face to face with one of the difficulties arising out of the plan of the book. The official preparations are not mentioned in detail, so that we are apt to confound the effects of the liniment with those of the tincture, for the two are, so to speak, mixed up together. Dr. Ringer praises the liniment as a local application in certain pains of the chest, but points out a practical distinction—if the pains are in the muscle, then iodine is to be used; if in the skin, belladonna suits best, but he does not limit the application of iodine to such pains. Among the remarks on the good effects of iodine injections in abscesses, &c., is a curious misprint, *pyæmia* being written where empyema was evidently meant, yet the error is rather a serious one. He says, on the authority of others, that iodine sometimes succeeds in chronic rheumatic arthritis when iodide of potassium has failed. Under the heading of iodide of potassium there is an excellent description of the various forms of iodism, with one exception—with some people the use of the drug is followed by a severe bilious attack, and that too in individuals not otherwise subject to such complaints. Dealing with its effects in syphilis, the author does well to indicate its use in tertiary rather than in secondary syphilis; but he makes an exception in the secondary syphilis of children, where the bones become affected in a peculiar fashion, but this is really a matter of terms; most people would call the affection tertiary; at all events, it comes within the group of affections over which the power of iodine is well marked and well known. Physiological explanations and therapeutical results are somewhat curiously jumbled up in this article.

That which succeeds is on the bromides (potassium, sodium, ammonium). The first thing noted is their sedative effects on the soft palate, which we think are not sufficiently well known. Then follows an inquiry into the value of the bromides in whooping-cough and laryngismus stridulus; in both, the author says, the remedy does good when the disease is uncomplicated. One of the peculiar features of this book is here well represented. Sometimes the author forgets that he is talking of the remedy, and runs into the treatment of the disease. The remarks are invariably sound, but the effect on the reader is somewhat peculiar. One kind of disease which he refers to as being readily controlled by a bromide is not generally recognised; it is a kind of colic in children, where the intestines form a sort of knot or ball, which travels from one part of the abdomen to another. After referring to the effects of the bromides in convulsive affections, Dr. Ringer describes their effects in other diseases. This portion is for the most part a repetition of what has been written by the late Dr. Begbie. He refers to the eruptions connected with these salts, but calls them small boils; more properly, they are acneiform spots.

The next section deals with the mineral acids; the most important remarks are those with reference to acids in dyspepsia, the author showing that, to do good, an acid should be given after, an alkali before a meal, that is, as an aid to digestion; but if to check acidity, the acid should be used before food. Sulphurous acid and the sulphites are next treated; most here written is after the exaggerated notions of the value of these remedies not long ago prevalent. Experience has, however, shown that, used as spray, sulphurous acid is useful in inflammatory sore throat, for it speedily causes blanching of the reddened tonsils and mouth, and used with equal parts of water it constitutes a nice, clean, pleasant enough gargle. Dr. Ringer speaks of the use of sulphites against sarcinæ; for this purpose, the acid itself is better adapted. Dr. Lawson speaks of it as valuable in pyrosis, but, except where sarcinæ or other parasites have been present, we have not seen it of much use.

The following group is an extensive one, uniting all the various alkaline substances belonging to potassium and sodium. This article is interesting in many ways, especially so, perhaps, in its key-note, which consists in the theory that acids stimulate and alkalies lessen alkaline secretions, the reverse being the case with those of an acid nature. Now, generally this is not exactly true, as we know from experiments with regard to the saliva (about which, by the bye, Dr. Ringer says "nothing is known." *Will* he take the trouble to look up Kühne's 'Physiologische Chemie?'). Certain substances have the

power of promoting the flow of saliva—acids, bitters, alkalies, pepper, &c. Now, let us take one gland, the submaxillary, which is supplied by nerves both cerebro-spinal and sympathetic. By stimulating the former a plentiful clear thin fluid saliva is produced; so with acids. By stimulating the sympathetic, a less abundant but thick opaque saliva is the result; so with alkalies. With regard to the parotid, again, its secretion can be readily stimulated through the cerebro-spinal nerves, not at all through the sympathetic system. Accordingly, also, in the case of this gland, alkalies and red pepper have no influence in inducing a flow of saliva. From this we may learn that alkalies do stimulate the flow of saliva, but only by virtue of their action on the sympathetic. Undoubtedly alkalies constitute powerful stimulants to the acid gastric juice. Another curious fact, to which Dr. Ringer does not give sufficient weight, is the one especially insisted on in German physiological therapeutics, the paralysing effect of potass salts on the heart. There is a reference to the subject, but only as an abstract of Dr. Paul Guttman's well-known researches. Under this heading there are also some remarks on the subject of diuretics, which are, perhaps, rather out of place. Dr. Ringer does not hold with the view that rheumatism may be cut short by alkalies.

The alkaline preparations of ammonia constitute Dr. Ringer's next group. They do not seem to be great favorites with him; he speaks slightly of them as used locally, and not very highly when given internally. There is, however, one diseased condition they speedily remedy, and in which the sufferers are very thankful for relief. There is a certain form of headache, as from mental worry or fatigue, and such like causes. A bottle of smelling salts held to the nose will often effect a speedy cure. When, again, the sputa of bronchitis are excessively tenacious, ammonia seems to us to do much good. Of the magnesian group not much is to be said, only Dr. Ringer appears to overlook the consequences of his theory as to acids and alkalies. When magnesia is taken into the stomach it acts as an antacid, and will not only absorb but stimulate the acid gastric secretion. The compound so formed (magnesium chloride) is a stable compound, and will not, as Dr. Ringer seems to think, become converted into magnesium bicarbonate in the intestines. Dr. Ringer overlooks the change in the stomach. The alkaline lime group comes next. As to their local application, Dr. Ringer speaks much more highly of greasy remedies than of drying powders in intertrigo, where the latter are so frequently used. He speaks very highly of lime-water in vomiting, especially among young children, where, indeed, its value cannot be too highly extolled. Milk and one eighth of lime-water is the

remedy prescribed. The phosphate of lime is treated in a separate section. No doubt this is right, but the question is whether the section should have not been made a very much longer one; in short, whether the volume should not have dealt with *foods* as contradistinguished from *medicines*, for it is to the former group phosphate of lime belongs. Dr. Ringer recommends it in cases of debility in women from over-childbearing, and in cases where there has been much mental exertion. The chlorides follow—those handled are the sodium, potassium, and ammonium salts, but the most important is that of ammonium. This remedy has long enjoyed a high reputation on the Continent, but has only recently attracted much attention in this country. Dr. Ringer seems to think that the special fields of operation for these salts are the mucous membranes; but this is, perhaps, too limited a view to take of the operation of ammonium chloride, and we do not think that he lays nearly enough stress on its effects in relieving headache.

Next comes an article on the purgative salts—the sulphates, tartrates, and sodium phosphate. Dr. Ringer adopts the view that these purge, not by increasing or creating an exosmotic current in the bowel, but rather by retaining the water they encounter there. In place of either of the simple salts above referred to, he advises the use of Püllna or Friedrichshall waters, a recommendation we cordially endorse, having for some time been accustomed to employ these alone in actual practice. Püllna water is said to be rather stronger than the other, but the difference is not great; an ordinary wineglassful generally suffices for a dose. Another practical hint is well worth remembering, viz. that several small doses will frequently succeed when one large one has failed. This is well seen in the case of mineral waters, and should be remembered in the treatment of torpid bowels, as from lead poisoning.

Of the nitrates not much need be said, except that Dr. Ringer does not value them highly in rheumatism; in this view we coincide. We may further add what the author does not, we think, sufficiently insist on, the fact that nitrate of potass, if given in large doses, is a very poisonous salt. Of potassium chlorate Dr. Ringer says, "What influence, if any, it has on the organs of the body is unknown." We quite agree with him. Alum is strongly recommended in the vulvitis of children, in certain forms of ulcer in the mouth, in chronic ozæna, and when given internally in whooping-cough. The author holds, apparently, that the salt acts by constringing, and, so to speak, deadening the throat in the latter complaint.

Next comes iron, which, as is remarked, "must be regarded as an important food." Dr. Ringer says also that the salts of iron are

not used as astringents in the mouth, as they are liable to be turned into sulphides by "the sulphuretted hydrogen of the breath." Surely Dr. Ringer does not wish us to believe that the gas in question is a normal constituent of the expired air. Another curious statement is that the quantity of acid in gastric juice is not great; this we can scarcely understand if we reflect that the juice, although not so highly acid as in many animals, is secreted to the extent of 30 lb. a day, according to Grüne-waldt.

The author has been accused of a fondness for excessively small doses of medicines. When dealing with bismuth he has shown a wise divergence from this rule, if, indeed, it can be called such. Speaking of the effects of the nitrate in the diarrhoea of phthisis, he says it must be given in half-drachm or even drachm doses. He says nothing of the dose for stomachal derangement, but we would seek to enforce the same usage there also. Bismuth is useless in small doses, invaluable in large ones. The article on lead is, to a great extent, taken up with the phenomena of acute and chronic lead poisoning, and with its influence on gout. We confess we cannot entirely concur with Dr. Ringer's remarks on the causation of lead poisoning, and on the absorption of lead by the skin. Of nitrate of silver the author does not appear to think very highly; he says that for sore throat glycerine or tannin is better. In this opinion we cannot altogether coincide, as we look on solid nitrate of silver, judiciously applied, as the most generally useful application in the majority of sore throats. Dr. Ringer speaks of its application as spray when dissolved in nitrous ether; this plan we have tried again and again, but cannot get the solution to keep.

The article on mercury is rather a long one. Dr. Ringer speaks first of its local effects, and highly praises its efficacy in removing the itching of many skin affections. He likes an ointment containing a drachm of calomel to an ounce of lard. He also speaks most highly of the effects of the bichloride (one grain to half pint of water, one drachm every hour) in certain diseases of children, especially slimy diarrhoea. His exposition of the principles of giving mercury in syphilis are, we think, good, and commend themselves to general approbation.

Of the article on copper nothing need be said. Of that on zinc we would remark that the author does not believe in the antispasmodic action of its salts except they nauseate. He recommends oxide of zinc in two-grain doses for colliquative sweating.

Antimony and arsenic complete the list of metals. In the former the most noteworthy observations refer to its use in a

form of disease occurring in children from six to twelve years old. After exposure to cold they are seized with a loud wheezing and some difficulty of breathing, and this affection may last a long time. Dr. Ringer recommends what appears his favorite dose, one grain of tartar emetic to half a pint of water, a teaspoonful for a dose every hour. He recommends the same in the bronchitis of adults. Arsenic, again, he strongly recommends in one-drop doses (we suppose of the liquor arsenicalis, although it is not so stated), for certain irritated conditions of the stomach occasioning vomiting. A considerable portion of the article is occupied with an account of arsenic eating and arsenical poisoning.

There is not much to be said as to the chapter on oils, except that it is sound, and the remarks as to the giving of cod-liver oil are worthy of general attention, for this substance is frequently most injudiciously administered. Neither is there anything very unusual in what refers to castor oil, croton oil, and glycerine, except that Dr. Ringer holds to the belief that alkalies increase the efficacy of the former two.

Next come the different kinds of tannin, a remedy for which Dr. Ringer appears to entertain a high respect, especially in the shape of glycerine of tannin, as a local application in ozæna, snuffles, discharges from the ears, eczema, throat affections, gonorrhœa, and internally in certain forms of diarrhœa, bleeding, &c. The tar compounds and derivatives succeed. This article is, of course, largely occupied with an account of Lister's plan of treating wounds, &c., which is too well known to need further remark.

Alcohol, chloroform, and ether come next, and a separate article is devoted to each. He deals with alcohol as an aid to digestion, as a means of diminishing oxidation in the body, as a food, as a stimulant to the heart's action, its use in fevers, the effects of different forms of alcohol, and, finally, as a means of diminishing heat. Chloroform, again, is considered with regard to its local effects, as in cancer, itching, toothache, colic, cough, and finally as an anæsthetic. Ether as a local and as a general anæsthetic, whether simply or combined, is briefly disposed of. The author seems to believe in the efficacy of iodoform in certain painful affections, as cancerous sores, but does not appear to speak from his own experience. Camphor is recommended to be sniffed up in colds or influenza, and it is said that these diseases may be thus arrested at an early stage. Dr. Ringer also speaks highly of camphor as a remedy for summer diarrhœa or even cholera; he prescribes six drops of a strong alcoholic solution every ten minutes. (This, by the bye, is the so-called Rubini's specific for cholera.) Turpentine is strongly recommended, and we think most justly, as a remedy for intestinal

hæmorrhage; small doses, frequently repeated, to be given. The same treatment will be found efficacious in other kinds of hæmorrhage from other organs. Its use in fevers is not overlooked.

The two articles on cantharides and mustard are very good. Of late there has been raised an outcry against counter-irritation, on the ground that no physiological explanation could be given of the good, if there was any, which it did. Most people were, however, content with practical experience, and most people have practical experience of the good of counter-irritation. Dr. Ringer's remarks on the way of using blisters should be borne in mind. He is all for *flying blisters*, not for vesication; the reddening does good, the derivation of serum weakens, and the sore causes pain. As a rule, therefore, he prefers mustard to cantharides, as giving the stimulation without the depression. One curious use of cantharides he recommends in acute Bright's disease, just when the inflammation has ceased. Here, says Dr. Ringer, one-drop doses every three hours do much to increase the urine, and to diminish the albumen and blood. It is one of the drawbacks in the system adopted in this book, that no opportunity is afforded of recommending one preparation rather than another. For our own part we must prefer the strong solution of cantharides to the ordinary plaster as a blistering agent, and we believe we shall universally be borne out in recommending to general use what are called "mustard leaves," as more cleanly and more convenient than the ordinary poultice.

The article on ipecacuanha has probably induced more hostile criticism than all the rest of the book put together, for the author maintains the value of single-drop doses of ipecacuanha wine as a remedy for vomiting. Rank homœopathy is the cry, and Dr. Ringer's fondness for minute doses elsewhere has given apparent strength to the accusation. Now, the way out of this muddle seems to us very simple. These small doses of ipecacuanha wine either do or do not remedy certain kinds of vomiting, and that they do concurrent testimony seems to prove. As practitioners, men whose duty is to heal the sick, but not to frame convenient hypotheses, it does not matter to us if homœopaths assume this as an instance of their favorite doctrine; it is our business to make use of the information, regardless of whatever theory is by a certain class attached to it. The only principle we have to hold by is the paramount necessity of doing our best for our patients. If one-drop doses of ipecacuanha wine do good, why not employ them?

Veratrum viride and album are chiefly considered with regard to their influence over pneumonia. The accounts of their

virtues are somewhat conflicting, and colchicum, of course, is viewed as affecting gout. Dr. Ringer adopts Dr. Garrod's somewhat speculative view, "that it controls the gouty inflammation." Podophyllum and its resin have been investigated by Dr. Anstie, whose researches are given here. Dr. Ringer recommends it in a form of constipation in children, indicated by hard, dry, crumbly motions. He recommends the resin to be dissolved in alcohol, and given in sugar. We are inclined to prefer a solution in ammonia, some spirit being added. The author recommends oil of stavesacre as a remedy for phthiriasis. *Actea racemosa* he strongly advises in rheumatic arthritis. We have no great experience of the remedy, and are accustomed to rely more on cod-liver oil. He also, but not from his own experience, speaks well of it in certain uterine affections.

Aconite follows, and, to judge from the terms in which it is spoken of by Dr. Ringer, it is to be the remedy of the future. As a means of relieving inflammation, taken sufficiently early, it is apparently beyond all praise, and in such affections as quinsy its effects can be seen; in colds it is also extremely valuable, and so in the nephritis which follows scarlatina.

The article on *digitalis* is a long one, but a considerable portion is occupied with physiological explanations. Dr. Ringer seems to hold the view that *digitalis* is a cardiac stimulant; at all events he knows, as do we, that it *steadies* its action, changes it from a short, sharp, and ineffectual contraction, to a longer, stronger, and more complete evacuation of its contents. And in certain forms of disease, more especially in dropsy depending on cardiac inefficiency, this is all important. Dr. Ringer prescribes the infusion in drachm doses, but frequently gives more. Of the use of tobacco in disease not much is said, and the author overlooks an important use of the remedy, as soothing an irritable organic muscle, such as the walls of the bladder. A pipe of strong tobacco will often afford complete relief from that troublesome condition; as nausea comes on the pain goes. Much of that which is said of *conium* is also physiological. Many people rely on *succus conii* in the hacking cough of phthisis. Dr. Ringer does not seem to say much for it under such circumstances. The article on Calabar bean is almost purely physiological, whilst that on *belladonna* is quite the reverse. The author praises its local application for the arrest of sweating, and of milk for threatened abscesses, and leucorrhœa, and subcutaneously or internally for neuralgic pains, asthma, whooping-cough, and incontinence of urine. Hydrocyanic acid is briefly treated.

The three most important articles which follow are on opium, *nux vomica*, and *cinchona*. We regret we cannot do more than refer to them, on the ground of space. Like most other por-

tions of the book, they have a clear, practical tendency, one of the most marked peculiarities of Dr. Ringer's book, and one which cannot be too much commended. There are two good fresh articles on poultices and enemata.

And now to sum up. This is a capital book, but at the same time with a little more care it might have been much better. We shall try to show how. One of its most marked characteristics is a want of exact method. It is apparently intended that each substance should be considered with regard to its action on the skin, its action on the mouth, the alimentary canal, and the general system, with, finally, its mode of elimination. But this most judicious programme is imperfectly adhered to. Sometimes each of these has a heading, sometimes it has not; sometimes one or more are overlooked entirely. Then, again, the physiological action of remedies has no definite *locus standi*; indeed, we consider this the weakest portion of Dr. Ringer's book. Whether or not such notices are necessary to the plan of his book is for him to consider, but having determined on them they ought to have a position of their own. One of the most marked features and most important and valuable peculiarities of the book is the notices of various diseases, especially of those of children; but when dealing with them Dr. Ringer constantly forgets the plan of his book—he deals with the disease rather than the remedy. Certain portions of the volume, therefore, resemble a chapter on clinical medicine rather than one on therapeutics. We are far from condemning this practice, but it should not be done in the body of a work whose symmetry it mars.

In literary merit the work is not altogether deficient, but is susceptible of improvement, and Dr. Ringer would seem to have been unfortunate in his printer. Such mistakes as *morbis cordis* for *morbus cordis*, *two* for *too*, and *lactate* for *lacteal*, instances detected at random, should not occur.

We have been careful and impartial in our remarks on Dr. Ringer's work, for we esteem it highly; indeed, in many respects it is by far the most practical treatise which can be put into one's hand, but it has defects—defects which, we doubt not, a second edition will speedily enable Dr. Ringer to remedy.

III.—Vaccination in Spain and her Colonies.¹

(Continued from No. lxxxviii, October, 1869, p. 339.)

"What a glorious enterprise! I have made peace with Spain, and quite adore her philanthropic monarch."—EDWARD JENNER.²

IN continuation of the account given by us in our October issue of the past year, of the expedition of Balmis from the shores of Spain to America and the Philippines, and his arrival home, we take up the thread of the story by following the second branch of the expedition under the sub-director Salvani, to the southern continent of America. To Salvani was appointed the task of diffusing vaccine throughout the viceroyalty of Peru and South America generally—a duty stoutly and vigorously carried out for many successive years, amid the pressure of countless perils and fatigues, at great personal risk to himself, and with no small cost of life to other members of the expedition.

Parting from the Caraccas, on their way to Peru, it was the lot of the expedition to suffer shipwreck at one of the mouths of the Magdalena river, but happily they were rescued. Salvani and his three colleagues, with the nurses and children, remained for a time in Carthagena, industriously engaged in diffusing vaccine in that city and its neighbourhood, with a very happy result. They likewise sent some to the Isthmus of Panama; and after sojourning on the banks of the Magdalena river so long as was necessary for the general attainment of their purpose, they proceeded to visit the towns of Teneriffe, Mompox, Ocana, Socorro, San Gil y Medellin, the valley of Cucutan, the cities of Pamplona, Giron, Tuaja, Telez, and other important places.

In these parts, according to the affirmation of the viceroy, they communicated vaccination to no less than 50,000 persons, and left directions for securing its extension and permanence. On the last day of March, 1805, they once more departed on their route, each one having allotted him a different track of country, so as to embrace as many inhabited spots as possible within the viceroyalty and in the Carrera of Papayan, in Cuenca, and Quito, as far as Lima; and in the August following they arrived in Guayaquil.

¹ *El Pabellon Medico: Revista Cientifica y Professional de Medicina, Cirujia y Farmacia.* Se publica los dias, 7, 14, 21, 28 de cada mes. Vol. viii, 1868. Madrid. *The Medical Pavilion: Scientific and Professional Review of Medicine, Surgery, and Pharmacy.* Eighth year. Published weekly. Madrid, 1868.

² This quotation we have copied from an original letter of Dr. Jenner written to Mr. Phillips, afterwards well known as Sir Richard Phillips, and dated Cheltenham, January 16th, 1807.—Ed.

The increased importance of many of these places since the commencement of the century warrants our giving a more detailed account of the expedition, and it is fortunate that we are able to do so from the medical report or service sheet of D. Manuel Julian Grajales, sub-inspector of the board of *Sanidad militar*, happily a survivor of the branch of the expedition under Salvani. He was charged by his immediate chief with a section extending from Mompox by land to Santa Fé de Bogotá; while Salvani himself followed the river Magdalena, directing his course towards the same city, where they met at the end of December, 1804. They again pursued their task over new ground, with a rendezvous at Papayan, where they arrived in April, 1805; the section under the orders of Grajales having traversed the mountain of Quindin. Once more they started separately from Papayan over a new region. Grajales proceeded to the province of Los Pastos, and by the mountains of Babacoas to the banks of the Jelembi, by which stream he travelled as far as Usmale, and thence by sea to Tumaco. From thence in a *piroque* he went to San Buenventura, continuing by the river San Juan to the river Tolu, as far as Choco.

Returning to San Buenventura he passed by sea to Gijón, Quito, Guayaquil, and Tumbes; and from thence through the unpeopled country of Mancora; at length reaching Piara, and having crossed the river Marañon, he passed on to Chachopayas. From this point again crossing the Amazon and the formidable Cordilleras of the Andes, the expedition, after a stay at Lambayeque, continued along the coast to Trujillo and other provinces of Peru, and arrived eventually at Lima. In point of fact, the various sections met in Lima shortly after the director, Balmis, had been received with honours on his return to Madrid—that is to say, in the end of December, 1806. In March, 1807, unsparing of fatigue, Grajales' companions resumed their labours, transporting the gift of vaccine to Cuzco, La Paz, and Arequipa; and in September they returned once more to Lima.

In the month of November following, notwithstanding the risk which Spanish vessels ran in these seas, from the war then carried on with Great Britain, Grajales sailed for Valparaíso, where he arrived at the end of December. In February, 1808, he persevered in his journey to Guasco, Coquimbo, and Copiapo; from thence to Santiago de Chile, where he arrived in May, 1808. The reasons which detained Grajales in this city till January, 1809, are not so apparent; we may conjecture, however, that they are to be found in the hostile disposition of these people, never very well disposed towards the Spaniards, and no less in the

sparse character of the habitations. Somewhat also may be allowed for hardships suffered, necessitating a recruitment of strength. Starting anew in January, 1809, Grajales journeyed to Talca, and thence to the province of the Concepcion. In September, 1810, he visited Valdivia; and on the road he communicated vaccine to the Araucanians. In 1811 he pursued his course to Osorio, proceeding as far as Villarica and the neighbourhood of the Straits of Magellan. He then went to Chiloe, and embarking at Caramapu, passed to San Carlos, port and capital of that archipelago; from this point, exposed to all the fearful risks of the sea in the winter season, he propagated vaccine as far as Guaylaz and Paylaz, situated in lat. 48° south. Lastly, he journeyed to the department of Calbuco, whence he was compelled to return to San Carlos, on account of insurrection of all these countries against the central government at Lima. In January, 1812, he proceeded to Lima, in order to give an account of his successes to the viceroy.

The lengthy detail in which we have indulged imposes upon us a befitting brevity of comment; it is impossible, however, to contemplate such an extent of labour, which from first to last occupied some years in its execution, committed to a few individuals, and carried out with such ardour and completeness, without experiencing a degree of admiration that partakes of surprise. The thoroughness of the work done, vaccination distributed and made a common thing, from 40° lat. north to 48° south; the journeying over 5000 leagues by land and 10,000 leagues by sea, in the region entrusted to Grajales alone; 400,000 persons subjected to vaccination, and stations everywhere appointed to secure a lasting supply—all this is calculated to raise an impression of a very different class from that derived from the details of a yachting excursion or the freebooting exploits of a Drake or Dampier. Nor does it want the element of pity to engage a further interest. Of the four professional and of the non-professional officers engaged in that portion of the work allotted to the sub-director Salvani, only one survived to see his natal shore,—no other than Grajales himself, to whose service-sheet we are indebted for this precision of details; all the others his companions fell victims to the labour involved, and to the pernicious effects of the climate of the countries visited, observing faithfully to the last the injunction of their sovereign that they should deliver these lands from the ravages of smallpox and spread vaccination through the country *without sparing cost or fatigue*.

No sooner was the successful event of the Spanish expedition to America made known through the press of Spain to other European countries, than it was most unhesitatingly accepted as an act

of the highest humanity. Husson, the intelligent secretary of the central commission in France, declared, in the most public manner possible, that until then there had happened nothing in the annals of vaccination that could any way compare for interest with this humanitarian expedition projected by the King of Spain. In England and Italy it was no less applauded as one of the most brilliant exploits in the history of Spanish kings. The Spanish laureate of those days, the great Quintana, stirred by a patriotic impulse, found lofty and immortal words commemorative of this occasion, which are still read with pleasure in an ode that keeps its place in Spanish literature. From this time, as regards the future, no other country seemed so rich in promise of all the best results of vaccination as the Iberian peninsula; a new crusade had been proclaimed, and smallpox, if not expelled from her territory, might at least be driven to the foulest nooks and corners and most neglected portions. But at this time other pestilences walked the earth in the form of an utter corruption of principles and of vicious perversions of thought. The shameless interference of Napoleon in the political system of Spain produced social disorganization, and a general insecurity in the length and breadth of the peninsula. The war of independence dragged its length along, and so it happened that from the year 1808 a long period followed, in which was beheld not so much a paralysis and inefficacy in all previous organisation as a total uprooting and destruction of all that had been previously designed for the safety and welfare of the people.

The result was that, in a very few years' time, Spain, carefully planted with local vaccine institutions, indoctrinated with ideas of this hygienic propaganda, had become so dull in all her instincts as to the preservation of human life that she became dependent on a single individual, and he of little note, for the continuance of vaccination in her territory. Against the devastation of smallpox there survived but one faithful foeman. A physician of Cascante, in Navarre, a warm and enthusiastic soul, Gil y Albenoz by name, well deserves a wreath of immortality for unceasing exertions made by him to maintain the succession of vaccine during the war of independence. In a recital afterwards published he tells how fruitless many times were his endeavours to wrestle against the sorrow and despondence, the changeful fortunes, the terrors, the spirit of recklessness and neglect engendered by these years of war. In 1814 he recounts how, by the most unwearied exertions, he managed to have always in his possession a sufficient quantity of matter in crusts, on glasses, or on points. By means of the *Gaceta de Madrid* he placed his store at the disposal of the country,

especially in places where there was present danger from smallpox. "One can scarcely imagine," he says, "what countless and earnest applications were directed to my door for supplies of this preservative in that unhappy period." During the year 1814 there literally existed in Spain no other established source of vaccine than that which his diligence maintained. On the 10th of November of that year, in laying before the royal supreme junta of medicine a summary of the results obtained by him, and condoling with numbers of physicians who had failed in procuring a supply, he remarked that, "During the thirteen years that I have given my mind to keep up vaccine uninterruptedly, there have been in Cascante three visitations of natural variola prevailing in all parts of the city. Three other times it has found its way in among us, one might say by stealth through infected children, just as we have seen it in Cadiz, 1800, 1804, 1812, 1813, conveyed by destructive germs from various localities, the West Indies amongst the rest, according to the well-skilled Flores. Just so in this city the variolous germ found its way in, for we never could have perceived it in the aeriform medium by which it spreads so as to keep it from among us, but finding no pabulum here, nearly every child having been submitted to vaccination, only twenty-two took the complaint, of whom but three died, according to the certificate of the parish priest annexed. Since the introduction of vaccination and the epidemic of natural smallpox in the year 1799, 1630 have died from all causes in Cascante, of whom only three died from smallpox, instead of 291 at the old rate of mortality from that disease. We have here a clear gain of 288 souls in a population of 3000 inhabitants. Now, the national census of 1799 gives 10,535,975 as the population of Spain, from which number there would have been saved by vaccination practised as I have made it here effectual, 1,011,453 individuals, or a tenth part of the population."

But in the year 1814 another witness, Sr. D. Felix Gonzalez, was even more explicit: he speaks of the existing precautions as feeble (*debiles*), and wholly unworthy of Spain and of the age in which he lived; and then he asks: "What have we done for the present generation in this thing of first importance? *Nothing*, he answers, *nothing*." The royal schedule of 21st April, 1805, he considers as virtually defunct, and in the present existing circumstances hardly realisable. Vaccination was all but abandoned; and those ideas among the people that were previously planted with so much care, had become not so much unsettled as uprooted, disorganised, destroyed, and cast to the winds.

We should desire to give a more precise account than we have

it in our power to do of the causes that have led to the present disastrous condition of Spain with respect to smallpox. Of these causes the principal, no doubt, has been the notorious instability of the various ministries and governments which have followed in rapid succession from the era of the great peace. We are able to discern far less clearly the operation of the existing machinery for the control of the variolous infection than those points of history that precede. For some time after the peace, 1814, it would appear that the department of vaccine fell under the care of the now defunct body, the *Supreme Junta of Health*, which certainly rendered many services to vaccination. Though far from approving such an institution without great and radical reforms, Dr. Montejo speaks more approvingly of it than he does of its successor, the Council of Health, a mere consultative body, which now has charge of the department. Its languid existence, apart from the general government, the secrecy in which it veils its operations and discussions, the negative character of its recommendations, are faults that lie at the root of the present disastrous situation. Though the Council includes among its members individuals of capacity and zeal, there somewhere resides a check, the springs of which are hid from sight. Though the best of measures are proposed, the result is next to nought. No more painful position for Dr. Montejo can well be imagined than this, to share a common opprobrium through the dalliance or obstinacy of those who have to be always waited for. It is no better with the provincial and municipal boards of health; they deal with infinitely small matters, but this of vaccination does not fix their attention, and seems to lie beyond their scope. As remedial to the present state of things, Dr. Montejo points to the influence the official academies of medicine existing in the Peninsula might bring to bear upon public opinion; he does not understand or approve their entire exclusion from this hygienic domain. The separation of the vaccinal department from all the public hospitals is what he particularly laments as so contrary to one of the provisions of the royal schedule above mentioned. He speaks of the efficacy of this early provision, of its good effect on the patients and pupils, of the debt which vaccination owes in time past to the officers of these establishments, among whom D. Juan Azoala, first surgeon of the great hospital at Madrid, especially deserves remembrance. But, in fact, there remains no trace at present of this former invaluable connection between the department of vaccine and all the large hospitals of Spain, although it was found worthy of recommendation in framing the 'Novisima Recapitulacion.' The reason may be found in the fact that the administration of both fields of action remains in the hands of persons who are wholly strangers to science; and

yet the hospitals receive a vast number of smallpox cases, and are much subject to its devastations.

There remain the clergy, all but omnipotent over the prejudices of the vulgar; these are not, as Dr. Montejo thinks, sufficiently appealed to, or sufficiently enlisted in the cause. In the beginning of the century the movement received from the prelates, and all the dignitaries of the church, the most efficient aid. Why is this no longer so? In fact, all classes and powers should combine against the enemy, instead of which our author says, "there is now a general neglect;" a fact sufficiently remarkable, which results from committing to the hands of a few a subject of the general weal: none but the enlightened lament the defect; of that class he asks, somewhat despondingly, "how many are there existing in Spain?" Add to this a certain mental perversity displayed in a few individuals of the medical class. In taking account of what has been done in aid of vaccination in Spain, we must not omit mention of the thesis proposed in 1859 by the Academy of Medicine in Madrid, 'On the Advantages and Disadvantages of Vaccination and Revaccination.' The result did not entirely answer the expectations of the Academy, and the prize was not adjudicated; nevertheless, two 'Mémoires,' one by D. Augustin Marion de Ovieta de Bilboa, the other of D. Cayo Peyrani, of Turin, received the honour of an *accessit*. Under the circumstances it seems a pity that the subject has not been reopened. Besides this, the Valencian Medical Institute, a voluntary association of medical men, created a special local commission of vaccine, having for its object the preservation of the fluid by arm-to-arm vaccination, and also its supply on application. The vaccine matter thus kept up has a fair repute, and is sought after from all parts of the Peninsula. In this point of view the Valencian Medical Institute in Spain holds now the same position there as the Jennerian Institution in London, and the Imperial Academy of Medicine at Paris.

The provincial Junta of Alava was roused to action by the occurrence of smallpox in Madrid and neighbouring districts in 1866. In the work of D. Geromino Roure, *Noticia historica*, recently published in Vittoria, the highest value is attributed to the labours of this provincial Junta; nor are the authorities and notabilities of the province one whit less deserving of praise. Like praise is also due to the governor of the province of Toledo and to Dr. D. Francisco Miguel Cuadrado, by whom the march of the epidemic 1867-8 was successfully controlled. In this part of Spain, unfortunately, there at present exists, *without distinction of class*, an unconquerable and vehement prejudice against the employment of vaccination. It

may be shortly stated that these provincials, far blinder than the savages of the Visayas in a past age, persist in rejecting this sure means of safety when decimated by a typhoid and confluent character of smallpox; refractory alike to authority and all recommendations of science. Much might be said, too, in praise of the medical officers of the municipal hospital in Madrid; ample advertisements in the public prints give notice of the days and hours for gratuitous vaccination at this ancient Spanish institution. The value of the plan is strongly attested by the officers of the *casas de socorro*, who visit the poor in their homes. The violent epidemic that reigns annually in that city during the whole of the winter months no longer affects, in frightful disproportion, the poor as compared with the rich; both classes seem to share in it alike, and this approach to equality is attributable no doubt to the well-sustained operation of this municipal institution.

We will now say a few words as to vaccination in the Spanish armies.

At the close of the year 1812 there entered the military hospital of San Fernando several soldiers affected with smallpox. The chief physician, D. Serapio Simces, then happening to be at Cadiz, applied to the general of the fourth army to have all soldiers submitted to vaccination in whom it had been neglected when young, provided they had not suffered from variola. On the following day the general applied for permission to the war minister, and readily obtained it by a *royal order of the 3rd January, 1813*, that vaccination should be performed in the armies of Spain on all soldiers who should present themselves for the operation, and without the use of compulsion. This was the initiative of progress. Long years after this (in 1831), D. Florencio Gomez and D. Pedro Alonso y Valencia, physician and surgeon respectively of the military hospital of Badajoz, made official request to the *ordenador general* of the army of Estremadura for vaccination to be performed on the soldiers, as protection for the ravages of smallpox, which had already begun to spread alarmingly in the garrison. On this solicitation there followed the *royal order of August, 1832*, which determined that vaccination should be performed, by relays of twelve at a time, on all soldiers who did not show marks of successful vaccination, or some traces on them of smallpox—soldiers to be exempt from duty until the crusts began to dry. Vaccination was also insisted on before admission to the army and military academies.

In April, 1843, the junta for direction of the *Sanidad militar* applied for a re-issue of the previous royal order which the recent civil war had made of none effect; and in December,

1848, the same junta decreed the fulfilment of the above royal order, with the addition that soldiers should be revaccinated who had undergone vaccination in early life, and *also those who had suffered from smallpox*. This order was renewed in November, 1851, an exception being then made for those who had suffered from smallpox, and was again confirmed by a further royal rescript in May, 1835, reiterating former provisions for vaccination and revaccination of the troops, and giving it a firmer foundation. These data sufficiently proved a certain growth and advance in the scientific idea, zeal on the part of the medical corps, and ready appreciation at head-quarters; but, sad to say, they fell far short of execution, or even of that approach to perfection which the service now exhibits. We shall allow ourselves a few words on this head.

It happened in the year 1844, in Navarre, that the chief of the *Sanidad militar* made application to the *Direccion* for some vaccine *virus*, wherewith to carry out the injunctions of the royal order, 1843. The Supreme Junta of Health in Spain, when applied to for the purpose, forwarded to the *Direccion* certain glasses, together with the assurance that they contained the real vaccine matter as received by them every month, and *taken immediately from the cows of the Philanthropic Jennerian Society of London*. Such a title sufficiently displays the high and almost superstitious esteem in which this institution was held.

Owing to frequent failure of the vaccine matter, and the prevalence of smallpox, the Minister of War, in 1858, subscribed for a monthly supply of the matter, for the ordinary exigencies of the service, from London. Accordingly, from July, 1858, such a regular supply was provided for and distributed to sub-inspectors, from time to time, according to the necessities of the hour. The medical officers of the army who watched the operation of this vaccine made constant and urgent complaints to their immediate chiefs of its absolute sterility in results,—of the constant failure experienced by them in all attempts at vaccination with the matter furnished. Even with children of an early age, who were in best condition to receive it, success was far from frequent. In connection with this same subject it will be instructive to take account of what happened ten years ago in the Canaries, the scene of the first success of Balmis at an earlier date. In 1859, for more than a year previously, some cases of smallpox had been observed in the islands, not amounting to the proportion of an epidemic, but smouldering without intermission. The sub-inspector, D. P. Vergara, in this situation of affairs, finding no supply of vaccine in the island, made application to the *Direccion General de*

Sanidad Militar, and duly received from Madrid glasses and points of vaccine derived from the Jennerian Institute. Having some experience in the matter, he made his first trials on infants and children of early age; but out of many attempts not one single instance of success was accorded him in Santa Cruz de Teneriffe. To be perfectly sure as to the quality of the matter he had at his disposal, he despatched some glasses to competent hands in the neighbouring city of Laguna, where, fortunately, there was obtained in a child one single vesicle of pure vaccine. From so slender a beginning as this Dr. P. Vergara had the skill to extend vaccination over all the island, and to succeed in the vaccinating and re-vaccinating the troops. This recital will bring to recollection the previous frequent and fruitless attempts made in the beginning of the century to effect vaccination in the Canaries.

It is not wonderful, then, that in course of time the vaccine matter from London became considerably discredited. In April, 1863, the existence of a smallpox epidemic in the army, more frequent, more general, and more severe than before, stimulated the General Direction to issue strict injunctions for arm-to-arm vaccination, starting with the Jennerian vaccine in preference, but beginning, in the first place, always upon young children. The same instructions were repeated in March, 1865; monthly reports were demanded, with an exposition of the method employed and of all contingent circumstances. If the Jennerian matter failed, then other means were to be had recourse to of procuring trustworthy lymph. We may shortly state that, after being deceived for more than twenty years by a false confidence, or rather by false conclusions, there has been a return to first principles; the habit of dependence upon the Jennerian Institute has been broken in upon, which had proved a real misfortune to all remote from that centre. The facility with which the vaccine is distributed from the institute, the scrupulous regularity of its supply to and from the heads of civil and military departments in Spain, has left nothing behind it but the bitterest reproaches and regrets, for it has always been attended by failure (*esterilizado fatalmente*).

This prejudice in favour of English vaccine on the Continent has been handled in a lively vein by Prof. Warlemont, in a paper read before the Royal Academy of Belgium, 24th June, 1865. Speaking of the universal belief that English vaccine, as its name implies, is taken directly from cows affected with natural cowpox, selected for the service of its clients by the Jennerian Institute, he explains that it is human vaccine that is sold and distributed abroad, and that no drop of the original virus, as taken from the body of the cow, has ever left the English

shore. "What we obtain from London," he says, "is no better than what we get at home, provided we see the disease, and know it to be complete." There is a prevailing mistake on the Continent as to the frequency in England of vaccinia in cows; it is believed to be a very general complaint, and not, as it is in fact, a rarity. They suppose that in a country where cowpox is spontaneous and abundant, it must, necessarily, be more energetic and more efficacious in its properties.

The present head director of the medical department of the army, D. José Maria Santuch y Marengo, soon after coming into office, 16th October, 1866, named a commission for vaccination in the army. The circulars 19th April and 21st December, 1867, and the royal order 15th January and circular of February, 1868, complete a system of reform for the better vaccination of the troops. More than 20,000 individuals have been submitted to vaccination in the first seven months of the year 1868.

A large share of these papers by Dr. Montejo is devoted to the condition of vaccination in other countries, to the subject of animal vaccination, that of commixture of virus, &c., subjects which now everywhere engage attention and challenge discussion. We have interested ourselves simply in the historical past, turning that side to the light which seemed least to court the view; we have presented here a section which will be needed one day, perhaps, to make the whole figure complete. The official occupation of Dr. Montejo, and the very scrupulous character of his literary work, which, as leaving him but little spare time, or it may be other circumstances, perhaps, may have led him to adopt this scattered form of communication in a journal. We can only say that there is nothing ephemeral in the subject-matter, and that we hope one day to meet with the same material in a more compact and portable form.

IV.—Treatment of Stricture of the Urethra.¹

THE pathology, treatment, complications, and results of stricture of the urethra have usually claimed a large share of the attention of practical surgeons, but never more so than in recent years.

¹ *On the Pathology and Treatment of Stricture of the Urethra.* By Sir HENRY THOMPSON, Surgeon-Extraordinary to His Majesty the King of the Belgians; Professor of Clinical Surgery, and Surgeon to University College Hospital. London, 1869. Second edition. Pp. 430.

On the Immediate Treatment of Stricture of the Urethra, by the Employment of the "Stricture Dilator." By BARNARD HOLT, F.R.C.S., Senior Surgeon to the Westminster Hospital; late Lecturer on Surgery in the Westminster Hospital School of Medicine.

If we exclude the department of ophthalmic surgery, the literature and the practice of which constitute the most advanced branch of modern surgical science, the number of really excellent surgical monographs in the English language is not large. In the first class of these, however, we may place the writings of Sir Henry Thompson, whose views on the surgery of the urethra, the prostate gland, and the bladder, have now for several years influenced the opinions and actions of a large body of our profession. In the practical treatment of stricture of the urethra, Mr. Barnard Holt has also deservedly acquired a great reputation. Mr. Holt's work sets an excellent example to others. It is short, direct, and to the point. It tells the reader what he wants to know, and, what is even more laudable, it does *not* tell him what he does *not* want to know. There are other writers, too, who have said and done things worth knowing and imitating; but English surgeons are unable to recognise, or even to see merit where the possessor has not yet reached eminence.

While much has been done, there is still much to do. So long as competent men differ, and differ widely, as to the most appropriate of the numerous methods of treating stricture in any given case, we cannot say that surgical inquiry has proceeded sufficiently far in this direction. Dilatation, "continuous" dilatation, incision-external or internal or subcutaneous, rupture, extreme stretching, and caustics—these are some of the methods of treating stricture. Are they all to maintain existence, and if so what is to be the precise province of each?

There has not recently been much that is both new and true to record in the anatomy of the urethra. The higher class text-books on anatomy give most complete and careful descriptions of the structure of the urethra; and the only knowledge which is of value to the practical surgeon, namely, that which is acquired by dissection, may be readily obtained under the admirable, if not always happily expressed, guidance of Mr. Ellis. It would seem, therefore, that surgeons who treat of stricture of the urethra, or, indeed, of any branch of surgery, should assume that their readers are acquainted with anatomy, or if they really have anything new and important to communicate, it should be given in the briefest manner. Strictly speaking, strange as the statement may appear, there is no special relationship between the anatomy of the urethra and the *pathology* of its stricture. The whole of the pathological action which is necessary to the formation of a stricture lies in the sub-mucous connective tissue. Such action is, of course, inflammatory, and does not differ from the inflammatory action which is found on any mucous surface or in any mucous canal. If the

rectum or the œsophagus or the nasal duct be inflamed, the pathological action arises and proceeds in the subepithelial areolar tissue. Should the inflammation persist in any of these localities, new young tissue is formed, and this new young submucous tissue, as it grows older, shrinks, becomes denser, and occupies less space. Such a process going on around a canal, more or less completely, necessarily leads to stricture. It is quite true that inflammatory induration is found in the corpus spongiosum, but here it is merely an accident, and if it exist in the spongy body only, the submucous tissue being free, there is no stricture. We do not say the accident is unimportant because the addition of the accidental to the essential often increases the practical gravity of the essential. The mode of formation, however, of a stricture of the urethra does not differ from the mode of formation of a stricture of the rectum or œsophagus.

Stricture, as is well known, occurs almost invariably in the spongy portion of the urethra, and chiefly in two or three localities, namely, just in front of the junction of the membranous and spongy portions of the urethra, in front of the scrotum, and near the orifice. The deeper stricture is the more frequent, the more severe, the more difficult to treat, and often exists without the anterior stricture, while the anterior stricture rarely exists without the deeper. Is there any necessary relationship of stricture to the corpus spongiosum. We have already seen that, judging by analogy, there is not. Why, then, should stricture invariably occur in the spongy structure? Probably from local and mechanical, not from anatomical causes. Within the bulb is the commonest locality of stricture. Now, the bulb, situated on the resisting triangular ligament, and being also the portion of the urethra which is nearest the surface of the perineum, is most exposed to mechanical unrest, than which there is no more efficient cause of protracted inflammation. The mobility and unrest of all the urethra anterior to the triangular ligament are, indeed, too obvious to require discussion.

It is a melancholy peculiarity of our profession that there are surgeons and physicians of eminence, not sufficiently appreciative of modern pathological research, who boast that they are practical men, forgetting that all practice is based on some pathology or other, either old or new. Among pathologists the tissue-element pathology is becoming every day more widely accepted as more consistent with physiology and microscopic investigation, and as more explanatory of the phenomena of disease; but there is scarcely a single popular monograph in which the old exudation pathology is not made to do duty as it has done for so many years past. It is bad enough to take it for granted, as is done by all authors on stricture, that the great

vascularity of the bulb has something to do with the occurrence of stricture in that locality, when the fact is that the inflammatory process is always most prevalent and most destructive in the tissues of lowest vascularity. The subcutaneous tissue has few blood-vessels, but it more frequently inflames and more rapidly sloughs than skin which is highly vascular. Compact bone with scanty supply of blood rapidly dies when inflamed, while the well nourished cancellous tissue inflames slowly and dies hardly. There is no evidence, moreover, to show that the submucons tissue at the bulb (the real seat of pathological action) is more vascular than the submucous tissue on other parts of the urethra. This is not the worst. There is not a single author on stricture who has so much as hinted a doubt on the hypothesis that stricture is due to the exudation of lymph. While physiologists, pathologists, microscopists, and histologists, have given up the idea of the effusion of lymph in the tissues, a lymph which nobody anywhere, or at any time, has ever seen, the "stricture doctors" still adhere to the exploded doctrine with a more than lymph-like tenacity. The question is eminently a practical one. One excellent surgeon has actually stated it to be his opinion that stricture of the urethra may form from lymph exuded on the surface of the urethra. Mr. Hancock, who is certainly not behind his compeers in pathological knowledge, based this opinion on a case in which a small corner-like portion of apparently stricture tissue lay loose on the surface. Probably in this case a piece of mucous membrane had been lifted from its bed by previous use of instruments, and had not regained its attachment. It is more consistent with microscopic investigation and with our knowledge of pathological action elsewhere, to say that the process which leads to stricture is one which altogether belongs to the tissue elements (whatever they may be—cells or protoplasts, or formed, or germinal matter, or what not). There is a constructive inflammation; the old tissue elements produce new ones in abundance; these, if not dwarfed by treatment, flourish to such an extent as to impair the use of the canal around which they lie.

We have said this much on the pathology of stricture because this branch of the question has been strangely neglected. We turn now for a little space to the treatment of stricture, and here the activity of surgeons has been really wonderful. A great proportion of the stricture cases are, happily, not very severe, and are kept in fair order by various means of treatment. Of these we have little to say. It is the very tight stricture case, with or without false passages and other complications, which the genuine surgical mind regards with the most earnest attention. In strictures which are situated in front of the

scrotum we believe with Sir Henry Thompson that incision is the best treatment. At or near the orifice this treatment is especially successful. The writer has often found incision of a congenital small meatus, as well as of stricture, to cure gleet when all other measures have failed. An ordinary director and a narrow straight bistoury are better than the bistourie cachée represented in Thompson's lectures, the cutting of which is done by an ugly square unsurgical corner.

The treatment by rupture, as devised by Mr. Holt, is a perfectly safe procedure, and is followed by no ill result. When we say perfectly safe we mean, of course, as safe as any treatment can be. There are cases where the presence of renal disease, unsuspected and with no albumen on the urine, renders even the passage of a bougie a fatal operation. There is no safer treatment. Its range of applicability is also large. The stricture or strictures must be moderately tight, otherwise the instrument stretches and does not tear, which is practically a disadvantage, because the rupture of the dense submucous tissue is preferable to dilatation, however excessive. On this ground we are disposed to think that the dilating instrument of Sir Henry Thompson will not add to his justly acquired fame—it certainly appeared at an ungraceful moment. But neither is Mr. Holt's instrument so perfect as he supposes. The practical difficulty may be great, but to be quite perfect the instrument should rupture the stricture a little more without adding to the bulk of those portions of the instrument which are not in or near the stricture.

Are there any cases of tight stricture to which Mr. Holt's operation is not applicable? We think there are. Now and then every hospital surgeon meets with old neglected and extremely tight stricture. The general health is probably impaired. A goodly number of hands have tried the catheter, and the existence of false passages cannot be doubted. In such cases as these the treatment may be said to be half done when an instrument of any kind or of any material is passed into the bladder through the stricture. It is not always so. We have several times met with stricture where a fine French elastic, bulb-pointed bougie could be passed into the bladder (only with the kindly aid of chloroform in some cases), where a metal "one" or "two" catheter could not be passed, and where consequently Holt's instrument could not be carried through the stricture. The presence of a fine catheter in Mr. Holt's instrument is not of very great advantage. A little coagulated blood in the instrument, when its position in the true canal is unquestionable, will prevent the urine following. This is not all. The urine may flow, and the instrument may not be in the stricture at

all; it may have passed under the stricture and entered the urethra again behind the stricture. A new *second* opening into the urethra will *grasp* the instrument. Again, when there is, as there not infrequently is, a second stricture anteriorly, the evidence of the instrument being grasped is utterly valueless as a guide to the route it takes on its deeper progress. These objections to Mr. Holt's treatment, in exceptional cases at least, are not hypothetical, they are based on experience.

We have now arrived at the question, What is the best treatment for a more than usually tight stricture? In some senses the treatment we believe to be best is old, in others it is new. It is an old custom to keep a small metal catheter in the urethra, having once introduced it. It is a very painful and unsatisfactory proceeding, inasmuch as the straight portion of the metal catheter lies in the strongly curved portion of the urethra. There are very few cases indeed, if any, in which this treatment could be preferable to Mr. Holt's rupture treatment. But recently French catheters and bougies have been introduced into this country of a very different kind to the gum elastic instruments of this country. They are very pliable, but of perfect polish; they are conical (the conicity is wonderfully accurate and gradual), and probe- or bulb-pointed. They follow the course of the urethra very readily, and over and over again we have found a small French bougie pass through a difficult stricture, when with all patience a metal instrument could not be passed. It is only fair to say that in rare cases a probe-pointed metal bougie or catheter will pass when a soft one will not. It does not follow, because a French bougie can be passed, that therefore a French catheter can also be passed. A small French catheter is limp; and when the point has passed through the stricture, and more or less of the conical part, the instrument stops. In these circumstances we have adopted an expedient which has rarely failed. The fine-wire stilette (we are speaking of instruments not larger than the "one" or "two") is bent to "Thompson's curve," carefully passed down the catheter, the point of which has passed through the stricture; the point of the wire also of course passes through the stricture, and lends just sufficient firmness to the delicate soft catheter to enable the surgeon to pass it onwards to the bladder. Now, when this catheter is in the bladder, and, as we may be quite certain, in the stricture, it is perfectly easy and moulded to every curve. The catheter well fastened in, a few feet of fine india-rubber tubing is attached to it, which permits a constant flow of urine into a vessel at the side of the bed. This expedient, whenever a catheter is retained in the male or the female, keeps the patient dry and comfortable. It has been

resorted to for several years at the Queen's Hospital, Birmingham. Mr. Jessop reports that it has been found useful in Leeds, and, indeed, appears to be under the impression that the practice is new. In twenty-four hours the "one" or two is taken out, and a three or four or five introduced. Contrary to the advice of Sir Henry Thompson, we put in an instrument that quite fills the stricture, and have reason to believe that a better result follows. Now, a curious fact is to be noted; when the second catheter is taken out at the expiration of a second twenty-four hours, a large French bougie, equal to an English ten or even twelve, can usually be introduced, and the occasional use of the bougie is all that is necessary. We are thus led by experience to consider that, in very tight and difficult and complicated stricture, the method just described is the most rapid, the most certain to avoid false passages, and perhaps the least likely to produce shock, even if renal disease be present. The patient is kept in the house only forty-eight hours; and if the question of time be an urgent one, twenty-four hours of a small soft catheter permits of the efficient use of bougies subsequently.

It may be now laid down as an axiom that no treatment yet discovered, or (bearing in mind the pathology of stricture) likely to be discovered, gives immunity from relapse. To this rule the mode of treatment just described is no exception, any more, possibly a little less, than other methods.

For inveterate cases of stricture complicated with sinuses which refuse to close after ordinary, or at least non-cutting modes of dilatation, Symes' external urethrotomy still remains the best treatment.

Does there remain any kind of case in which caustics can be of possible benefit? We know the violence of the outcry against them. It may even be a just outcry, but we suspect it has in it a considerable leaven of the parrot. We know that Mr. Liston denounced them, but the time of day has come to scatter to the winds the baseless dicta of any authority, however brilliant or however enslaving his influence may have been in his own time and over his own pupils. Liston decried ovariotomy in the vehement language which strong men are apt to use; it is now not less recognised than other standard operations. His contempt for elastic instruments was withering; they are now preferred by those most experienced in the surgery of stricture. What the fate of caustics in stricture will be we do not pretend to say; but in rare cases of extreme tortuosity and considerable induration, and where it is just possible that an instrument cannot be passed, or when there is no time for prolonged and repeated efforts, a small piece of potassa fusa, or other escha-

rotic, carefully applied to the stricture in the axis of the urethra, is a mode of treatment which deserves a trial, in spite of Liston, prejudice, and denunciation. At any rate, the watchful and impartial surgeon soon learns that he must have several arrows in his quiver for the treatment of stricture.

V.—University of Pennsylvania.¹

THE assertion may be hazarded, with little fear of controversy, that the majority of scientific and professional men in Great Britain are profoundly ignorant of the history, constitution, and present position, and even also of the name and locality, in many instances, of the great educational institutions—the universities and colleges of the United States. There is a prevailing impression, that almost every large town in the States has launched into existence an institution bearing the important title of University, or, at the least, of College; that too great facilities and encouragement are accorded by the several States for the establishment of such institutions, and that, in consequence of the numbers of corporations empowered to grant degrees in the several faculties—and particularly in the faculty of medicine—and of the competition and rivalry among them, their educational arrangements are defective, their examinations superficial, and their degrees granted unworthily. The fact of the prevalence of such an impression will of itself explain much of the indifference and ignorance to be found on this side of the Atlantic respecting American universities.

But it must be remembered that the ready, and, as it would seem also, excessive up-growth of Universities is chiefly a consequence of the rapid development of the country;—the continual formation of new centres of population at remote distances from older ones, rendering the formation of new educational centres also a necessity. Moreover, the activity and ambition of the American mind are not satisfied with any establishment that seems wanting in name, character, and general features that may give it, at least, a resemblance to the like institution existing elsewhere, and regarded more or less as a model for imitation, and as an object of rivalry. State pride will demand an independent university, nominally complete in organization and functions, and thus raises an obstacle to the creation of great national institu-

¹ *A History of the Medical Department of the University of Pennsylvania, from its Foundation in 1765, with Sketches of the Lives of deceased Professors.* By JOSEPH CARSON, M.D. Philadelphia, 1869, 8vo, pp. 227.

tions which, by their importance, and the renown of their professors and examinations, might bring together the youth of many States, advance the progress of more profound science and literature, and tend to raise and consolidate the character and acquirements of the educated classes of the nation.

Another and less justifiable cause of the multiplication of colleges with powers to grant degrees, has its origin in rival doctrines both in theology and in physic. Denominational colleges are probably inevitable, and little harm can be done by dubbing men Doctors in Divinity, how little soever their divinity knowledge be worth notifying; but it is not so with medicine, and the readiness with which powers to confer degrees and licenses in that faculty are obtainable is a matter of much regret, as involving great social evils, and as destructive of the very object for which special examinations and diplomas are required, viz. to guarantee to the public the possession of sufficient skill and knowledge, in the case of those who hold them, in the exercise of the profession they follow.

Fortunately there are several admirable exceptions among the graduating institutions of the United States, not moved by cupidity to sacrifice the interests of the public and of science by fallacious certificates of competency; and several such that have long been favorably known and esteemed in Europe. Among such, and those chartered as medical licensing bodies, are the colleges and universities included in the recognised lists of the General Medical Council, and of some of our own universities. Amongst them also is the institution whose history is presented in the work under notice.

The University of Pennsylvania is one of the oldest in America, and it is with just pride that Dr. Carson, the present Professor of *Materia Medica*, recounts the history of its medical department, and that of many of its eminent teachers. The book he has produced had its origin in a lecture delivered by him at the opening of the course of 1865, which also happened to be the centenary anniversary of the establishment of the medical school. This lecture he has now greatly enlarged from the mass of materials previously in hand, and subsequently collected; the main source of information existing in the minutes of the Board of Trustees and of the medical faculty.

We are introduced to the circumstances attending the first formation of the colony at the close of the seventeenth century, and learn that "in some cases the theological and medical professions were united in the same individual," many of the Puritan clergy, prior to their emigration from England, having studied medicine as a precautionary measure in case of ejection from their livings. Hence it happens that there were several

medical works published in America, at that early date, by divines. The non-clerical doctors—at least the surgeons of the times—were best and usually known as barbers, and, judging from the payments allotted them, valued as such. In the middle of the eighteenth century this objectionable appellation appears to have been discontinued and replaced by ‘chirurgeon.’

The development and prosperity of the colony also attracted graduates in medicine, chiefly of the Scottish universities, some of whom became famous in connection with the colonial and municipal government. One, Dr. Kearsley, who died in 1772, left his estate to found an asylum for aged indigent females, and a Dr. Thomas Graeme became a justice of the supreme court. But besides emigrant physicians, the colony after a few years was served by many home-born medical practitioners, a large proportion of whom, however, not content with the limited means of education to be then found in their native land, resorted to Europe to study in London or Edinburgh, or in Paris or Leyden;—the last-named seat of learning being at this time famous on account of its renowned teachers, Boerhaave, Albinus, and Gaubius.

In 1751 the Pennsylvania Hospital was originated by Dr. Thomas Bond, who happily was ably seconded in his philanthropic purpose by the illustrious Franklin. In this institution was the first clinical instruction given by its founder. Its medical library dates as far back as 1763. As yet, however, no medical school had been formed in Philadelphia, though Dr. Cadwalader, who had been educated in London and studied anatomy under Cheselden, gave private lectures and demonstrations, and Dr. Shippen, a few years later, delivered a public course of anatomy, illustrated latterly by a set of plates and by casts prepared at the expense of the celebrated Dr. Fothergill, and presented by him, together with a donation of 150 guineas, to the Pennsylvania Hospital.

An interesting circumstance connected with the foundation of the medical school of Philadelphia is, that both the Dr. Shippen, above referred to, and Dr. John Morgan, who became the first professor of medicine, were pupils of the Hunters and of Hewson. Dr. Shippen, indeed, is stated to have resided in the family of John Hunter whilst studying in London. It was chiefly at the instance of Dr. Morgan that a medical department was established and attached to the College of Philadelphia, an institution which owed its origin to Franklin, and which had already existed sixteen years when the medical school was engrafted upon it. The school was inaugurated in 1765 by a lecture by Dr. Morgan, and very shortly after Dr. Shippen was appointed Professor of Anatomy. The staff was increased in

1768 by the election of Dr. Kuhn as Professor of *Materia Medica* and Botany; but the most noteworthy accession it received was in the person of Dr. Benjamin Rush, in 1769, as Professor of Chemistry.

At this period the medical school of Edinburgh was at its zenith, and the fame of Cullen attracted students from all countries. The first occupants of the chairs of the Philadelphia college were graduates of Edinburgh, and this connection with the Scottish university had a lasting influence upon what Dr. Carson terms its legitimate offspring—the Philadelphian school.

Rush was an enthusiastic admirer of Cullen, and re-published in America his “*First lines of the Practice of Physic*,” soon after its appearance in Edinburgh, adding a preface to it written by himself.

The war with Great Britain interrupted the prosperity as well as the activity of the Philadelphia College in all its departments, and led to the suspension of lectures; and at length, in 1779, the College was superseded by an institution entitled the “*University of the State of Pennsylvania*.” This absorption of the college was, however, not acceded to on all hands, and the dissentients ultimately succeeded in re-establishing the college with its medical school; and, in consequence, a rivalry, or, in fact, an antagonism arose between the two establishments thus called into existence, each exercising its privilege of conferring degrees.

“The state of things exhibited (writes Dr. Carson) with respect to medical teaching by two institutions, in so contracted a sphere as the City of Philadelphia then offered, could not be otherwise than unsatisfactory,” and the spirit of faction having been lulled by the lapse of years, an amicable adjustment was brought about, and a reunion of the two effected by an Act of the Legislature in 1791, instituting “*The University of Pennsylvania*.”

During the severance of the two institutions, Dr. Rush had faithfully adhered to the college, but on the reunion he readily took his position on the staff of professors, exchanging, however, the chair of chemistry for that of medicine. During all these years surgery and midwifery had been taught by the Professor of Anatomy, but in 1805 surgery was made a separate course, and allotted to the celebrated Dr. Physick, and, at a still later date (1810), midwifery also was detached, and its teaching delegated to Dr. James. The last-named physician continued to hold his professorship until an advanced age, and in 1824, on account of his increasing infirmities, an adjunct professor was appointed—Dr. Dewees, whose name is familiar to the present generation as a skilled accoucheur.

This brings us well nigh to the era of contemporaries. Dr.

Carson extends his history to the year 1830; but it would serve no useful purpose to follow him in the notices of the successive changes in the arrangements, and in the medical staff of the Pennsylvanian University School; not but that, by so doing, we should introduce the names of many physicians and surgeons who have acquired a European fame, and adorned their *alma mater*; we, however, have fulfilled the object in view in noticing this volume, viz. that of making better known to Englishmen the rise and progress of one of the oldest medical institutions in America; of showing its early and intimate connection with England, and particularly with Scotland; and moreover, the association with its prosperity of some of our celebrities in medicine—Fothergill, Cullen, the Hunters, and Hewson. The University has proved a success, and the medical department has been, as Dr. Carson relates, the parent of numerous schools of medicine, and a nursery of teachers. Altogether, nearly 8000 medical graduates are enrolled on its register. A table in the appendix, however, shows that, although frequent fluctuations have occurred in the yearly number of graduates, there has yet been no decidedly progressive increases. An average of the last twenty years would be about 150 per annum.

In reference to our preliminary remarks on American Universities, it would have been of interest to have presented a sketch of the existing regulations for medical degrees, but none later than those of 1811 are detailed by Dr. Carson, and we can hardly imagine the same to still continue in force, considering the great changes that have taken place as well in the social status of medical men as in the diffusion of knowledge among the public at large, and also the vast additions made to the boundaries of medical science. The rules referred to required the candidate for the degree of Doctor of Medicine to be twenty-one years of age, to have studied medicine three years, two of which must have been spent in this University; to have attended the Pennsylvanian Hospital one session at least, and to have attended the practice, and been the pupil of some respectable practitioner. Attendance, moreover, was required on lectures on anatomy and surgery, on the institutes and practice of medicine, on clinical medicine, materia medica, and chemistry. A thesis, also, was called for.

The University of Pennsylvania claims the distinction of having been the first institution in America to confer medical degrees—a claim that has been disputed by the graduates of New York. However, Dr. Carson seems to make good the claim of his university, showing, from the minutes of the board of trustees that ten gentlemen were publicly admitted to the degree of Bachelor of Medicine in June, 1768, although they

did not proceed to that of Doctor until 1771. New York University (the then King's College of that city) followed, conferring the degree of Bachelor in 1769.

Admitting the claim of the Pennsylvanian University to priority in this matter, we will conclude by expressing a wish that this comparatively venerable institution will be jealous of the honour of its degrees; will, by its teaching and by honest searching examinations, augment confidence in their value, and will, by force of example, induce other and younger American universities so to enhance the value of their degrees, that they may have a just claim to recognition abroad.

VI.—*Northern Archives of Medicine.*¹

IN our number for July, 1869, p. 161, we announced the appearance of the first part, and briefly stated the general scope, of the above Journal, intended to be the exponent of the progress of medical science in Sweden, Denmark, Norway, and Finland. The second and third parts have just reached us, and are well calculated to keep up the favorable impression created by the earlier issue of the 'Northern Archives.' The Journal is bi-lingual, the articles appearing in the Swedish or in the Danish language, according to their source. As in the latter language the German custom of printing the nouns substantive with initial capitals is followed, the Swedish and Danish or Norwegian articles are at a glance distinguishable.

The first article, by the editor in chief, consists of a history and detailed description of the splendid Pathologico-Anatomical and Medico-Chemical Institution in Stockholm, recently erected in the immediate vicinity of the Seraphim Hospital. It was Virchow who first carried out the erection of a special pathologico-anatomical institute, and the example has of necessity been followed. The Berlin institution has now been excelled by others in size and various conveniences, but not in internal life and activity. The principal institutions of the kind subsequently erected are those of Prague and Vienna. Professor Key was induced to undertake the present history and descrip-

¹ *Nordiskt Medicinskt Arkiv.* Redigeradt af Dr. AXEL KEY. Första Bandet, Andra, Tredje och Fjerde Häfterna. Med Planscher. 1869, Stockholm, Samson och Wallin, 8vo, pp. 599. [*Northern Archives of Medicine.* Edited by Dr. AXEL KEY. (With the co-operation of Committees in Copenhagen, Christiania, Lund, Stockholm, and Upsala.) First Volume, Second and Third Parts, with ten Plates; and Fourth Part, with four plates.]

tion chiefly in consequence of the difficulty he himself experienced during the preparation of the plan of the Swedish establishment, from the fact of the non-existence of published drawings and descriptions of any other similar institution than that of Berlin. There can be no doubt that the detailed description he has now given, accompanied as it is with very accurate plans and drawings, and full particulars of the expense, will be found most useful by those who may hereafter be engaged in similar undertakings.

The Thrush-fungus.—Professor C. Reisz, of Copenhagen, contributes an interesting article on the occurrence of thrush-fungus (*Oidium albicans* of Robin) on the mucous membrane of the stomach. It is well known that Berg¹ limited the occurrence of the thrush-fungus to the tessellated epithelium found in the mouth, throat, and œsophagus, believing that the parasite could thrive only in that kind of epithelium. Exceptions of this law have, indeed, since been met with; but some of these seemed rather to confirm the rule than to overthrow it. This is true of those cases in which the thrush-fungus has been found in the air passages, for in these instances it was almost always drawn in from the throat, and was consequently found lying loose in the mucus, without being adherent to the membrane. Only in rare cases was it found adherent, and then invariably in places where lamellar epithelium either normally existed, or had been pathologically developed. However, in 1863 Liebermeister found the thrush-fungus growing on the mucous membrane of the stomach of a girl only fourteen days old, and consequently under circumstances which made it probable that it was there developed during life.² This case seems decidedly to establish that the fungus may be developed in the stomach, and therefore renders it probable that at least some of the earlier reports of its occurrence in that locality deserve credence.

The case described by Professor Reisz occurred under the care of Professor With in the Royal Frederiks Hospital, in Copenhagen. The patient was a girl aged eighteen, who had until within the preceding half-year enjoyed good health. She had menstruated regularly from her sixteenth year, until six months before her death, when the catamenia ceased, she became pale and rather thin, and complained of pain in her chest and of dyspnœa. On the 9th October, 1868, she had shivering, pain in the abdomen, thirst, nausea and constipation; on the 11th she had bilious vomiting; on the 12th she was

¹ Fr. Th. Berg 'Om Torsk hos Barn.' ('On Thrush in Children.') Stockholm, 1846, p. 4.

² Communicated by Zalesky, 'Virchow's Archiv,' Band xxxi, p. 426.

admitted into hospital, in a semicomatose condition, the urine contained a trace of albumen; on the 14th she died comatose.

The summary of the post-mortem examination was as follows: "Gastritis aphthosa. Hepatitis et pancreatitis lobulares. Nephritis parenchymatosa (levi gradu). Pachymeningitis interna acuta (levi gradu). Hyperæmia piæ matris (meningit. incipiens) et cerebri."

The author remarks:

1. "That in this case *oidium albicans* was found developed on the mucous membrane of an adult. 2. That it may be assumed that it was developed during life. Thus the dissection was made twelve and a half hours after death, and the fungus was seen to exist in an excessive degree, and to an extraordinary extent, in the stomach, inseparably connected with the mucous membrane, into the superficial layer of which the spores had distinctly penetrated. These facts sufficiently prove that in this instance the fungus cannot have developed itself after death in the mucus covering the ventricular mucous membrane, a mode of origin which Berg hypothetically puts forward as a possible cause of its occurrence beyond the limits assigned by him to it. Nor can it be admitted that the extension of the fungus to the stomach was favoured by the presence of a pathologically developed lamellar epithelium, for there were here no traces of extensive cicatricès, while between the sporular and filamentous masses columns of the normal cylindrical epithelium of the stomach were found."

Since the author brought forward the foregoing case, he met with the account of a well-established instance of thrush fungus in the stomach. This was in the case of a child, aged fifteen days, who had suffered from greenish diarrhœa and aphthæ in the mouth. The history of the case was related at a meeting of the Société Médicale des Hôpitaux on the 10th of July, 1868,¹ by Parrot, who supposed that the fungus had been previously developed in the glands of the mucous membrane of the stomach. Gubler, who, as is well known, has distinguished himself by his researches on *oidium albicans*, remarked on this occasion that the occurrence of the thrush fungus in the stomach must be very rare, as he had as yet never met with a case of it.

Dr. A. Törnbom contributes a paper on Professor Lister's method of treating wounds and abscesses, and Dr. Sven Sköldberg one "On the Treatment of Ulcerative Catarrh in the Cervix Uteri."

District Physician C. Homann proposes the question: "Can we, from the frequency of nervous fever in Norway, draw any

¹ 'L'Union médicale,' 22ème année, No. 104, p. 328.

inference as to the existence of an adynamic morbid constitution?" It would be impossible in this short article satisfactorily to follow the author through his elaborate inquiries; we must, therefore, content ourselves with stating the conclusion at which he has arrived, viz. "that the facts observed in Norway, far from justifying the assumption of the existence of an epidemic morbid constitution as the cause of nervous (or typhoid) fevers, on the contrary, point in the strongest manner to another definite cause, namely, contagion." He justly adds that the causal elements of one of the most destructive epidemic diseases of the country, are of the greatest importance in a practical point of view.

The shorter communications to the second number are, 1. On the periphery of the tunica Descemeti and its influence on accommodation, by Hjalmar Heiberg, Assistant in the Pathologico-Anatomical Laboratory in Christiania. 2. On rotatory dislocation forwards of the bones of the forearm, by Dr. Holmer, of the Kommunehospital in Copenhagen. 3. On an abnormal origin and course of the superior laryngeal nerve, by R. Nordensson, Medical Student in Stockholm. 4. On aneurism of the ascending aorta, by J. Nicolaysen, University Exhibitioner in Christiania. 5. On suppurative pylephlebitis, depending on gangrenous separation of the vermiform process, by Professors P. H. Malmsten and Axel Key, of Stockholm. Reports on northern medical literature.

Our space will not admit of our doing more than enumerating the articles in the third number of this valuable Journal. The first, by Dr. G. A. Hansen, of Bergen, is on the characteristics of the *spedalskhed*, or elephantiasis Græcorum, or *lepra nodosa*. The second, by Dr. Georg Asp, of Helsingfors, has for its subject the minute structure of the liver. These two papers are illustrated with beautifully executed plates. The third, by Prof. Carl J. Rossander, of Stockholm, is on the causes of failure in extractions of cataract. The fourth, by Dr. Adolf Kjellberg, of Stockholm, treats of the existence of parenchymatous nephritis in infancy, as a complication of other diseases, with special reference to its concurrence with intestinal catarrh. The part concludes with the usual copious reports of the progress of northern medical literature.

The names and residences given in the two preceding paragraphs exhibit a pleasing union of different, though closely allied, nationalities, zealous in the common object of advancing medical knowledge and of promoting the welfare of the human race. We trust that this extensive and laudable co-operation will continue to produce as rich and ample fruits as have marked its inauguration and its earlier labours.

Since the foregoing was written, we have received the fourth part, completing the first volume of the Journal, containing some important papers.

Syphilitic Disease of the Nervous System.—Dr. Ernst Oedmansson gives, under the title of “Cases of Syphilis” (Syflitisk Kasuistik), an elaborate essay, extending to ninety-eight pages, chiefly on syphilitic diseases of the nervous system, an important and interesting subject, on which we dwelt at considerable length in the twenty-eighth volume of this Review, p. 285, when examining the works of Steenberg, Lagneau, Gros, and Lancereaux.

Dr. Oedmansson divides his essay into three parts; in the first he communicates “Cases of Syphilitic Encephalopathia; with a brief account of the Pathological Anatomy and Diagnosis of Syphilitic Diseases of the Brain.” The second contains “Mixed Cases.” The third is devoted to “Cases of Hereditary Syphilis;” under two heads—(a) “Changes in the Placenta and its Appendices,” (b) “Changes in other Organs.”

The author, having given a very valuable and interesting history of the literature of his subject, remarks that—

“A glance at the multiplicity of changes in the brain and its membranes, which are met with either separately or, as is usually the case, variously complicated with one another, clearly shows that the most different morbid forms may be produced by cerebral syphilis. Experience proves also that the latter may assume the form of almost all the ordinary diseases of the brain. A syphilitic tumour may produce the same symptoms as a tumour of another nature; syphilitic epilepsy, pachymeningitis, softening, &c., may simulate the same disease, depending on another cause. It is possible that, through continued investigations, some standpoints may be obtained for a differential diagnosis; but at present we are acquainted with no pathognomonic signs of cerebral diseases caused by syphilis” (p. 11).

Among the indirect elements of a differential diagnosis, the author enumerates the existence of unmistakable signs that the organism is under the influence of the syphilitic poison; decided signs, or the history of previous syphilis; the patient's age. Thus, an apoplectiform attack from any other cause, except, perhaps, emboli in cardiac disease, is rare before the thirtieth year, and not common before the fortieth; idiopathic epilepsy, on the contrary, usually begins earlier. Therefore, if these diseases occur at the ages mentioned, they ought to excite suspicion of syphilis, and lead to closer examination of the patient. Lastly, the result of treatment; under this head Dr. Oedmannson remarks:

“We cannot but agree with Rollet, when he says—‘I have never

met with paraplegia, epilepsy, or hemiplegia, without wishing, in the interest of my patient, to find syphilis as the source of his disease, precisely because of the much greater probabilities of cure which the affections of this nature present.' "

In speaking of hereditary syphilis, the author states that syphilitic changes in the placenta and its appendices have hitherto been but little studied; and that with the exception of some few cases, only imperfect reports are to be met with on the subject. Having referred to these, he adds:

"The foregoing are the principal observations of syphilitic changes in the placenta and foetal membranes which I have met with in literature. Their limited number depends, not on the rarity of the changes, but upon the fact that these parts have not generally been examined. Bärensprung ('Hered.-Syph.') is, indeed, an exception to the rule, for he has constantly directed his attention to the placenta. Usually observers have contented themselves with assuming that in the cases where the foetus has died in utero, and where no organic diseases have been discovered in it, death has been the result of the general syphilitic infection. Still less than the placenta and the membranes has the funis been the subject of investigation. Yet here, too, changes are undoubtedly often met with, which are of the greatest importance in estimating the cause of the death of the foetus in the womb. My statement that syphilitic changes, both in the vessels and the placenta, are not rare, I base upon the fact that among nine cases of syphilitic abortion, in which during the last two years I have had an opportunity of examining the organs in question, changes were found in five, both in the umbilical vessels and in the placenta, which, according to my view, were in all these cases the principal cause of the death of the foetus. These five cases, with another, in which considerable changes were discovered in the vessels, though the child was born alive and nearly at the full time, are appended." (p. 75.)

Intestinal Infusoria.—Walter Ekecrantz, of Stockholm, Licentiate in Medicine, communicates an interesting and, in connection with the recent observations of Prof. Tyndall upon "Dust and Disease," a highly suggestive paper on the infusoria occurring in the human intestinal canal. The first discovery of the existence of such parasites was made by Davaine, who, during the epidemic of cholera in Paris in 1853-54, met with masses of animals belonging to the class of infusoria in the evacuations of patients treated for that disease in La Charité. The animalcules, referred by their discoverer to the family Cercomonas, were observed by him, during the epidemic, in several of the cases examined, but have since been seen only once, likewise by Davaine, and were then contained in the

dejections of a patient with typhoid fever. In 1856 Prof. Malmsten, of Stockholm, found another infusorium in the evacuations of a patient suffering from chronic entero-colitis, which S. Loven, who gave a description of it, looked upon as most closely resembling the family *Paramæcium*, and for which he therefore proposed the name *Paramæcium coli*. In the following year Malmsten again saw these parasites in the case of a woman, who likewise suffered from intestinal catarrh, and whose ailment corresponded in many particulars to that of the first patient. They were next observed in 1863 by Dr. Stieda, of Dorpat, who also communicated another case, which occurred, in 1865, in Wachsmuth's clinique. Another animal of this class was observed by Dr. Lambl, of Prague, in the mucous dejections of children.

The author, in examining microscopically the dejections of patients in the Seraphim Hospital, succeeded in finding specimens both of *Cercomonas*, similar to those described by Davaine, and of the so-called *Paramæcium coli*. The former were found, in the summer of 1868, in the excrement of a cottager under treatment in the hospital since the beginning of March. He remained in the hospital for nearly a year after these animalculæ were first seen, and in all the numerous examinations subsequently made their presence could be demonstrated. That they existed at the time of the patient's admission is extremely probable, as the symptoms from which he suffered were, during the whole time, the same. The author met with similar animalculæ, towards the close of the year, in another patient. The patients, whose cases are detailed, both suffered from chronic diarrhœa; in the second, portions of tapeworm, one measuring four feet in length, were passed. The author gives an accurate description of the animalculæ; our space will, however, allow us to remark only that the time they survive, when ejected from the intestinal canal, depends in a great measure upon the temperature; if this be low, they die very quickly, but at from 60° to 66° F. they are found alive at the end of five or six hours, and if placed in the sun they may live even two or three hours longer. They do not bear the addition of water, being rapidly destroyed by it, but they live long in urine and saliva, for which reason the author used these fluids in studying them.

Hr. Ekecrantz adds a brief report of the five cases, including one of his own, in which the infusorium called, when first discovered, *Paramæcium coli*, has been met with in the human subject. The credit belongs to Leuckart of having demonstrated the constant occurrence of this parasite in the intestinal canal of the pig. The author gives a detailed description of the para-

site, and illustrates his very interesting essay with a well-executed plate, representing the *Cercomonas* as found by himself, by Lambl, and by Davaine, and also the "*Balantidium coli*," most observers being of opinion that the animalculæ in question should be referred to the family *Balantidium* rather than to *Paramæcium*.

Obstetrics in Copenhagen.—Professor Stadfeldt, of Copenhagen, gives a report of the working of the Lying-in Institution of that city, from the 1st of April, 1868, to 31st March, 1869. 1088 women were delivered; as 15 had twins, the total number of births was 1103, 488 having taken place in the institution, and 615 in its branches (*Filialerne*). Of the children, 576 were boys, and 526 were girls; in one premature delivery the fœtus was removed before the case came under the professor's observation, and the sex was not recorded. Of the women, 797 were unmarried, widows, or separated from their husbands; 297 were married;¹ 539 were primiparæ; the premature births were 230. There was one case of *uterus bilocularis*, with single pregnancy in one half. The malformation was discovered in consequence of the necessity for introducing the hand for the removal of the placenta. The operations were 80, consisting chiefly in the use of the forceps, which was employed once in 29 cases. The *puerperal mortality* was much greater than during any of the three preceding years. Thus, it was, in 1865-66, 1·7 per cent.; in 1866-67, 2·8 per cent.; in 1867-68, 2·3 per cent.; and in 1868-69, 4·7 per cent., 51 deaths having occurred in 1088 deliveries, or 1 in 21. This great mortality was due to an epidemic of puerperal fever in December, 1868, and January, 1869, which was remarkable rather for the intensity of the disease than for its extent. Of 200 parturient women, 56 were attacked, and 28 died. Two deaths from non-puerperal causes raised the total mortality of the institution for the year to 5·1 per cent., one woman having died of uræmia and convulsions two hours, and one of typhoid fever eight days, after delivery. The author was unable to discover the cause of the persistence and violence of the epidemic. It began in the institution itself, the sanitary condition both of the branches and of the city at large being good at the time. In January it spread to the branches. When it commenced the institution had been in use for two and a half months after the annual closing in July and August. In

¹ The married women in 1868-69 were only 27 per cent., against 31 per cent. in the three preceding years. Probably the large proportion of unmarried females delivered in the institution has some bearing on its great mortality; in the Dublin Lying-in Hospital the mortality is usually greatest among that unfortunate class.—REV.

September and October only one death occurred, namely, after delivery complicated with *Placenta prævia centralis*.

In the course of this epidemic carbolic acid was, for the first time, tried as a disinfectant in the institution. When used in solution of one part in fifty in the ordinary mode, its employment was attended with advantage, but when tried "in a more energetic manner, namely, as an injection into the uterus immediately after delivery," the result was so unfavorable that its employment in this way had to be given up. From the 9th to the 17th of January these injections had been employed in 15 parturient women; of these, 3 died of puerperal fever. The author states, on the authority of Grünewaldt, that injections of disinfectants into the uterus immediately after delivery had previously been advantageously used.

It would appear that the institution consists of two stories, each containing two corridors, upon each of which latter open eleven apartments, each apartment being destined for the reception, labour, and subsequent confinement of a patient. Believing that one medium of the conveyance of infection was the air in the corridors, and being convinced that infection almost always takes place, when it does occur, during the labour, Professor Stadfeldt tried the experiment of allowing the foregoing plan to be carried out on the second story, while on the first he set apart four of the twenty-two apartments as labour wards, well isolated, from which the patients were to be conveyed, in from half an hour to an hour after delivery, to the apartments in which they were to remain during the rest of their stay in the hospital. The result was that on the second story the mortality was 6·7 per cent., while on the first, with special labour wards, it was only 4·9 per cent. He admits that this difference may have been partly accidental, but argues that the subject is worthy of consideration, at all events in an hospital constructed on the corridor system. It appears to us that it would be better to allow at least two hours to elapse before attempting to remove the patient from the labour ward. In conclusion, Dr. Stadfeldt justly remarks that in the hygiene of lying-in institutions an efficient system of ventilation is the most important element, and he expresses a hope that the investigations entered upon by Candidate Krarup may lead to a practically satisfactory result.

The other papers in the fourth number of the Archives are a "Report of the Working of the Orthopædic Institution for the year 1867-68," by Dr. A. Tidemand, of Christiania; a paper by Dr. J. Hjort and Dr. H. Heiberg, of the same city, on the "Malignity of Virchow's Glioma;" one by Professor Axel Key, of Stockholm, on the "Reaction of Aortic Aneurism on the Heart;" and the usual valuable reports, under twelve heads,

of Northern Medical Literature, those in the present number being for the first quarter of 1869.

We are happy to see that the success of the 'Northern Archives' has exceeded the expectations of its projectors, both in the number and value of the literary contributions sent in, and in the extent of the subscription list. This is no more than we should have expected from the evident hearty co-operation of the numerous band of zealous and able observers whose names stand recorded in the first volume of the first Journal ever sent forth as the general exponent of medical progress for the now professionally united kingdoms of Sweden, Norway, Denmark, and Finland.

WILLIAM DANIEL MOORE.

VII.—Baby-Killing.¹

IN the early part of the year before last some curious inquiries were made by a physician in London as to the real meaning of a class of advertisements which have of late become pretty numerous in the columns of the daily newspapers. These advertisements, for the most part, are more or less euphemistically worded, and purport to offer to ladies requiring temporary retirement, not only suitable apartments, but careful nursing and the services of an experienced medical man. In most cases there is something added, to the effect that the baby may be left or adopted. The memory of the revelations thus made and published in the 'British Medical Journal' cannot yet have quite faded away. Together with the facts elicited from time to time by inquests upon infants dying in what have of late been termed "baby-farms," they have served to lift a corner of the dense veil which conceals from the sight of decent and respectable people the hideous vices which underlie the polish of our modern civilization. Pharisaically hugging ourselves in satisfaction at our superior morality, we refuse habitually to have our equanimity disturbed by any suggestion that, after all, the outside crust may be a very thin one, and that by breaking it we may discover that we are little better than our neighbours. So long as we can ourselves lead peaceable lives, obtain all that we desire, and pursue our avocations and pleasures without hindrance, we of the easy sections of the community are very apt to give little thought to proceedings about us in which we suppose we have no concern. We charitably bestow a

¹ *Six Reports of the Coroner for Central Middlesex. Various Pamphlets and Papers.* By Mrs. BAINES. *ACTON on Prostitution.* Second edition.

sigh over the evil of the world that we hear about, or see flaunted in our eyes if we chance to wander out after dark. We are, possibly, roused to something like indignation at some flagrant cruelty reported in the newspaper, but our sentiments of pity or anger rarely carry us much further; certainly there are few who, taking up the clue which is offered, follow the thread as it winds through the maze constructed by human depravity. It is to the credit of our profession that, brought by our daily duties more than any other class of persons into relation with the sins and follies of the world, while ministering to those who suffer from them, we have done our utmost to arouse public attention and to direct public effort towards the application of a remedy.

If it be said that, as medical men, we have nothing legitimately to do with moral delinquencies, that we should leave them to be investigated and dealt with by the politician and jurist, we reply, first, that in so far as they have any operation in the production of disease and in increasing mortality, they ought not to be overlooked by a class of observers intent on searching out the causes of each, with a view to mitigation; and secondly, that such delinquencies occupy so prominent a place, and form so often the ultimate issue of etiological inquiries, that we could not ignore them even if we would. Moral and physical evils are so inextricably interwoven that it is out of our power to separate them, except by doing violence to truth and nature. In laying bare, therefore, as we now propose to do, what we esteem some main causes of the unnecessary destruction of infant life in this country, we shall limit ourselves by no restrictions of this sort, but shall follow in the track of those writers, both private and official, who have considered that their position as medical men was no valid reason for shutting their eyes to facts which commend themselves more particularly to the consideration of the social reformer.

In the course of the five years, 1863 to 1867, the Registrar-General has recorded, as having occurred in England and Wales, 578,394 deaths of infants before they had attained one year of life. He does not enlighten us as to the distribution of these deaths through the several periods of this short space of existence; but those persons who have access to the original registers can satisfy themselves of this fact, that at least half of the total number of these short-lived beings come to their end before the expiration of three months from their birth. We may even venture a step further, and say that, counting back from the end of the first year to the earliest weeks of existence, there will be observed a progressive increase in the number of infants whose deaths go to swell our mortuary tables.

Of those beings who meet with their death in the womb, or during the process of expulsion, which we designate as "still-born" infants, we have in this country, unhappily, no record whatsoever. This is one of the blots on our registration arrangements. There is some reason to believe that they would be found to constitute between 5 and 6 per cent. of all births. It is said that this is the ratio in the Netherlands, which is regarded by Dr. Elisha Harris as about the normal rate in civilised countries. Probably in large cities, such as London, it would be found to be greater. In New York the ratio is as high as between 17 and 18 per cent. But as regards our own country, as we have shown, we can only speculate. Human nature is much the same everywhere; and when there is no fear of detection, the temptation to a criminal neglect of those precautions which the accoucheur takes to preserve the life of the child during the act of parturition is, probably, too strong to be resisted in many cases where opposing motives have little cause for existence.

Of children stillborn in Italy, in 1864, the illegitimate births were more than three times as numerous as the legitimate. We have before us the six annual reports of the coroner for Central Middlesex; and we find that of 136 inquests held on the bodies of new-born infants, in as many as 119 a verdict of stillborn, or prematurely born, was recorded. In each year the majority of these were bodies which had been thrown away and picked up in the streets. Dr. Lankester says—

"If the question is asked as to whether any moral guilt is implied in the death of these stillborn children? I answer at once that there is as much reason to suppose that their lives have been sacrificed as there is of those who breathed after they came into the world. The facts on which I rely are the following:—In the first place these children are found with the cord untied, and all other marks of the neglect of any attendance upon the mother during her delivery as in the case of the liveborn children. In the next place they are found deposited in streets, gardens, and quiet places, under the same circumstances as liveborn children. We are, therefore, driven to the conclusion that they are the offspring of women who have had no assistance at their labour, and who have concealed the birth of their offspring" (4th report, p. 8).

Suspended animation is no rare event for the newly born infant; and where no end but shame is to be gained by the life of the infant child, it may readily be left to pass into actual death. That, when this last happens, it is in most instances the result of the absence of proper care, seems to be shown, not only by the ordinary success which attends the efforts of the accoucheur to resuscitate such children, but from some remarkable facts

recorded by observers on the Continent which indicate an occasional capability on the part of the newly born infant of retaining vitality under the most unfavorable conditions. We allude to instances of newly born infants having been actually interred as dead, but resuscitated after remaining in this condition for several hours, recorded by Maschka, of Prague, in 1854, and by M. Badinet, of Limoges, in 1865. It is observable that in one of Maschka's cases, notwithstanding the resuscitation, the lungs, when the child died, as it appears soon to have done, presented no evidence of having respired. "It cannot be doubted," says Caspar, referring to cases of this kind, "that a child possessing only this pseudo-life can be killed passively as well as actively, by omission as well as commission." All such omission, of course, is not criminal, some of it may be purely the result of accident; much, probably, is the result of ignorance; but it is difficult to clear our minds of a very strong suspicion, if not of a belief, that very much is of a different character. Until our registration laws are amended, and the registration of all births, whether of live or dead children, is rendered compulsory; until more strict statutory regulations are framed, by which the surreptitious disposal of the bodies of newly born infants may be impeded, we shall never know for certain how often the fruit of illicit connections is blasted at the very birth. Let us add, that the absence of such laws in this country can serve no purpose except the concealment of crime and the promotion of demoralisation.

Passing, however, now to a point at which our national statistical records become available, we find that we may compare the 578,394 deaths of infants under one year with 3,730,613 living births, this number being made up of the annual births from 1864 to 1867 inclusive and the mean births of the two years 1862, 1863. The comparison informs us that, of the children born during this period in England and Wales, 15·5 per cent. died in the course of their first year.¹ This is the mean for the whole of England; but, as manifestly violent deaths are not excluded, or deaths the result of ignorance or carelessness, the 15·5 per cent. cannot be regarded as a normal infant death rate under circumstances where such causes are not operating. We desire to know what death-rate is to be regarded as inevitable, under tolerably favorable circumstances, for infants up to the end of the first year. The Society of Friends in this country forms a community whose infant death rate may be usefully considered in this point of view. During

¹ It is evident from this that Mrs. Baines greatly exaggerates when she says that from forty-three to forty-five infant deaths take place in every hundred births.

the five years 1863-7, in the families of the members of this small society, there occurred 147 deaths of infants under one year of age, which may be compared with 1323 births computed as before.¹ This will give a death ratio within the first year of life of 11.1 per cent. In these families there are none really destitute, marriages are usually contracted with some regard to prudence, and, as a rule, not at a very early period of life, while domestic training gives fair prospect of care being bestowed upon the young. If we may accept their ratio as a normal death rate under favorable conditions, we arrive at this, that the infant death rate of England is nearly half as large again as it ought to be, and have next to inquire how it is that in the five years referred to 164,190 deaths happened more than there was due and proper cause for happening. Were it possible to make a strict inquiry into the circumstances of the death of every infant, it is scarcely to be doubted that among those whose deaths we are now going to enumerate would be found the items which go to make up the excess.

Deaths the causes of which were not specified when registered	12,458
Sudden deaths, the causes of which were never ascertained	3,879
Deaths from violence	8,398
Want of breast milk	6,595
Diarrhœa	54,274
Thrush	5,060
Atrophy and debility	107,825
Teething	10,459
Convulsions	105,294
	<hr/>
	314,242

We shall not be considered as extravagant in asserting that half of these 314,242 deaths, at least, ought not to have occurred at all; that they would represent roughly, for the five years referred to, the amount of unnecessary destruction of infant life due to active or passive criminality, ignorance, and carelessness.

But we do not propose to discuss the mode of origin of these causes of infant death separately. If we did so we should probably find that, for practical purposes, the terms under which these deaths were registered were mutually convertible. That the deprivation of breast-milk, from which between 6000 and 7000 infants died, was, in the vast majority of instances, a violent interference with the operation of a natural instinct, and not the result of maternal incapacity; and that the diarrhœa, thrush, marasmus, and convulsions were, with few exceptions,

¹ For these numbers we are indebted to Mr. Charles Hoyland, the recording clerk of the society.

associated with either entire withdrawal of the breast, or with the substitution, in part, of improper food, such as the infant organs are neither fitted for nor capable of digesting or assimilating, with neglect, or with drugging. We have made these few cursory and preliminary remarks merely with the view of showing, in the rough, the magnitude of an evil which we ought to deplore and try to remedy: in pursuing our subject we propose to limit ourselves to considering the relation to it of two social faults, namely, the voluntary abandonment of maternal duty (but for different reasons) by the rich and the poor, and irregular and immoral relationships between the sexes resulting in illegitimacy of offspring.

We shall say very little about the position of those women whose young infants are sacrificed to the necessities of labour. Their case is often a sad one; it is a story which repeats itself daily in our manufacturing centres where female labour is in request, and it has been told over and over again. There is nothing new about it. The life of a female factory hand is something such as this. She commences to work at a very early age, as soon, indeed, as she can be of use to earn a little money. Formerly she used to be taken from school as young as seven or ten years of age, but the Factory Acts now require that her schooling shall not be entirely interrupted, and for some years she spends her time between school and the factory. When emancipated from the former, her time is all devoted to work, until the evening, when she usually finds amusement with her companions in the factory of both sexes. If she does not fall into evil ways, and not seldom if she does, she marries or settles in one way or another early. If she is a good hand, and earns high wages, she is not unlikely to be picked up by some lazy fellow on the look-out for a young woman whose earnings will keep him while he drinks his brains away in the beerhouse. Anyhow, her marriage makes no difference in her work, except that, in addition to her factory occupations, she has her home to see to, her own and her husband's clothes to wash and mend, and meals to prepare. In due time she becomes pregnant, but still has to go to her work up to her very confinement. Can it be wondered at that such a woman is unfitted to be a mother?—without any knowledge of domestic economy, and without any of that experience in the management of an infant which she might have acquired at home in different circumstances, or with a sort of experience which is worse than none at all,—the traditionary treatment of infants which she has seen pursued in her own family under similar conditions. The period of enforced rest is all time lost; a month or six weeks pass, and she is again at the factory; her

earnings are regarded as too valuable to be dispensed with. What, then, becomes of the baby? It is treated just as its mother was treated before it. No doubt it is a wrench at first to find that the baby must be left all day in charge of some old woman or child who is glad to earn a few pence for taking care of it; but time abates this feeling, and it is succeeded by more or less indifference, the very death of the child being, perhaps, regarded as a relief. Such a woman may take charge of the infants of several factory hands, and the latter have been seen early on a winter morning hurrying along to the nursery, in their way to work, carrying through the cold and sleet their half-dressed infant, huddled up with its clothes and the food for the day. Where, then, are the comforts and tender cares that the infant of a few weeks old absolutely lives upon? The only time that the child can be suckled is in the intervals of work at meal times, when the mother has to get her own food, and when home for the night. At other times the infant must be fed artificially by the woman or child in charge. Those who have watched this feeding tell us that the food mostly consists of bread softened and made into a pap with water and a little sugar; and even this is not smoothly made, but presents the appearance of lumps floating in the water, being hastily prepared and given at any temperature that may chance, and as quickly as the infant can be got to swallow it from a spoon. Milk is too dear, and besides, there is a tradition among some mothers that when a child is partially suckled cow's milk is likely to disagree. The process of feeding, in short, and general management are much the same as may be observed any day among the labouring poor in London, and in day nurseries, where children are similarly left while the mother is at work. For the system referred to is not confined to our manufacturing towns. It may be seen in operation in any parish in the metropolis. We are ourselves quite familiar with the sight, and also with that of the puny, pale, old-looking babies lying in their cradles, in the dark, sickly-smelling, unventilated attic. That the little beings, deprived of the cosy warmth of a mother's breast, of the only food that they can properly digest and of the little cares and caresses that a mother only can bestow, should suffer from this treatment is nothing surprising. Flatulency, indigestion, diarrhœa, thrush, convulsions, and the like host of infant maladies, become their almost inevitable portion; and at night the fractious infant is lulled to rest by a dose of Godfrey or other narcotic, that the wearied parents may not be deprived of the sleep that they require to prepare them for the next day's labour. It is not unlikely that the dose at night is often not the only one that the child gets in the course of the twenty-four

hours ; for a room full of crying children could not be borne with when so simple a domestic remedy as an opiate or a little gin will send them off to sleep.

In a paper by Dr. Greenhow, a few years ago, it is calculated that at least ten gallons of Godfrey's Cordial were sold in Coventry alone every week, a quantity equal to 12,000 ordinary doses ; and a druggist in Nottingham, whose word could be relied upon, stated that he himself sold 400 gallons of laudanum annually, of which he believed that one half was administered to infants. And thus these infants are nursed to death. It is no matter of astonishment that there are many factory hands who have lost nearly all their children in early infancy, or that, when evil days have come upon their trade, and the mothers have been thus forcibly detained from work, the infant mortality has been observed to fall !

We have no hope that in these days of fierce competition female factory labour has any chance of being dispensed with. The establishment of public day-nurseries, under proper and intelligent supervision, is calculated in a degree to mitigate the evil ; but more is to be looked for from the charitable indulgence of masters, furnishing opportunities for the proper periodical suckling of the infants, and from the spread of domestic knowledge among the hands themselves, who should be taught that true economy lies in the mother's performance of home duties, and in learning to promote the comfort of the husband and children by making the most profitable use of their earnings.

But let us turn to another class. What is to be said of mothers whom no such imperative necessity can be supposed to bind, and who abstain from performing their maternal duties that they may be free to spend their time in the frivolities of fashionable society ? Probably a luxurious life is not the best preparation for suckling an infant, while the capacity of the mother to afford proper and sufficient nutriment is lessened by everything which stimulates the nervous functions at the expense of lowering the bodily powers. Such mothers as these, and some who, without their excuse, decline to suckle their infants because they do not like the trouble, commonly depute the task to a wet nurse. Some, we are ready to admit, take this step for the only valid reason, namely, that if they did not the infant would have to be brought up by hand. The practice of putting out infants to wet-nurse is not followed here to the extent that it seems to be in Paris, where from a very early period (it is stated ever since the year 1350) laws have been framed for the regulation of nurses, the protection of the child, and the avoidance of imposition. In fact, in France, such nursing is a recognised business, which can only be carried on

under the supervision of the police authorities. Out of a mean of about 53,000 infants born in Paris, 18,000, or one in every three, are sent into the country to be nursed, by far the greater number through the agency of some dozen offices for nurses established in that city. M. Husson states that from 1839 to 1858 the mean mortality of the nurse children under one year, placed out and looked after by the "*Direction des nourrices*," was nearly 30 per cent., and from 1859 to 1864 nearly 34 per cent. Three fourths of these infants were legitimate, and so under the most favorable conditions for living. The mortality among the "*enfants assistés*" was very much greater, namely, nearly 56 per cent. between 1839 and 1858, descending, however, in 1864, to 39 per cent. This was in the department of "*la Seine*." In other departments, where the children are not so well looked after, and where the infants of this class are not brought up as they ought to be at the breast, the mortality has ranged from 59 to over 90 per cent. It has been observed that the greater part of the nurse children die in the course of the first week of removal. With us the practice of wet-nursing is carried out in a different manner. The wet-nurse is usually taken into the family, and so is kept more or less under the supervision of the mother, and in this way some dangers to the life of the infant are avoided.

The propriety of "wet-nursing," under any circumstances, has been a subject of much discussion. The objections to it that have been raised are mainly these, first that it is immoral, and secondly that it is unnecessary. A very energetic and well-meaning lady, Mrs. Baines, has taken infinite trouble to impress her opinion upon this subject upon English women and medical men; and in summarising her views, as expressed in a variety of papers and pamphlets with which she has favoured us, we shall probably succeed in stating nearly all that can be said against the use of a wet-nurse. If a wet-nurse be engaged, either because a wealthy mother cannot or will not suckle her own infant, not one only but two infants are deprived of their natural and rightful nutriment—the infant of the nurse and the nurseling. With respect to the former there can be little question that invariably, or almost invariably, the nurse's child is brought up artificially, and that it depends very much upon the wages received, and upon the possibility of confiding its care to a careful relative, whether or not the artificial feeding will be a success, or whether upon it the infant will pine away and die as dry-nursed infants so commonly do. With respect to the latter, we imagine few medical men will be found to agree entirely with Mrs. Baines. Of course the best thing is that the infant should be suckled by its own mother, but we can hardly regard

the deprivation as a serious matter as regards the nutriment simply, so long as her breast is merely replaced by that of another healthy woman. Mrs. Baines, our readers probably know, has a "theory" to support her in this opinion of hers. We will quote her own words. "The milk of a stranger is not the fitting substitute. '*No other milk than the mother's can suit her own child, and her milk is not proper for any other child.*' This peculiarity belonging to individual milk is not to be discovered by an analysis of its elements; these resemble each other, with little variation, in different specimens of the same kind of milk; and by a chemical comparison of the elements in the milks of different animals, the relative proportions of these elements are found scarcely to vary at all; but the milk of each individual—speaking more particularly of human milk—possesses its own peculiar *quality*, or *essence*, or *power*, which cannot be detected by any known processes of analytical chemistry." Mrs. Baines adds, "This *theory* of a subtle existence in milk will be received, I am aware, with considerable caution, perhaps with much mistrust. It cannot but be expected that so novel a proposition will be questioned; the possibility of such an 'essence,' however, cannot be denied, for however remote the final decision of the question may be, who amongst us can say without presumption, 'it is not so?'"

A truly feminine line of argument, about which our chivalry will permit us to say no more than this, that possibly the author of the "theory" might find a sufficient explanation of any difference in result of feeding from the mother's or a stranger's breast in the different amount of care bestowed by a mother or a nurse upon the infant, in the sentiments with which it is regarded on either side, and on some consequent or associated modification in the quantity and readiness of the supply, perhaps even in its quality, such as chemical analysis *might* be able to determine. The influence of emotion upon the secretion of human milk is well known. If another explanation be needed it might be, perhaps, occasionally found in the very different kind of life led by a nurse introduced into a respectable family to that which she has been accustomed to. Mrs. Baines considers that it is wrong to tempt a woman by money to forsake her own baby, and sees no qualifying, no extenuating circumstances in the case of an unmarried woman thus selling her milk. In a question of casuistry we may presume to differ somewhat from Mrs. Baines. It has been urged that to a girl who has an illegitimate baby, a path of return from vice is opened to her by employing her as a wet-nurse, "as though," says Mrs. Baines, "that were the *only* employment available as a means of escape from future sin. But numerous other occupations offer, which it is not

necessary to enumerate.” We think that we have a right to ask for their enumeration, for Mrs. Baines has to show us how a girl, so situated and forsaken by her friends, can maintain herself, and also devote time to the care and nursing of her infant. We confess we are not so sanguine or clear-sighted as this lady. Let her read Mr. Acton’s book on the causes of prostitution, and she will learn how difficult it is for a friendless girl in London to maintain herself alone by such labour as she can obtain without quitting her home. We confess that, for our own part, we can conceive the hiring of herself to be a wet-nurse as a very venial fault, if a fault at all, while we can regard it, at other times, as a deadly sin. It is assuredly the latter if a girl, anxious to be rid of her offspring, or careless of what becomes of it, places it where she has no good reason to know that it will be properly brought up; and if the girl’s hirer, thoughtless of all interests but those of her own infant, so selfishly uses the natural heritage of another as to bring about, thoughtlessly, its untimely death. On the other hand, the magnitude of the fault on both sides is evidently greatly reduced where the hirer, as bound by morality to do, conscientiously busies herself to care for the bereaved infant, sees that it is properly placed out, carefully and judiciously fed and nursed, and, should occasion arise, is restored to its own mother. We condemn, as loudly as any one, the cold-blooded manner in which it is but too customary to transact the business of hiring a wet-nurse. If hirers of wet-nurses would only act as conscience must tell them they ought to act in this matter, the principal moral objection to the adoption of the system would be done away with, and a real good might be occasionally done to a girl sometimes more sinned against than sinning. We have said that we believe wet-nursing to be often adopted very improperly and unnecessarily, but there are instances where it is the only remedy applicable for the preservation of the life of an infant deprived by any cause of its mother’s breast. With such precautions as we have indicated, we think that medical men are in such cases perfectly justified, both on medical and moral grounds, in recommending the hire of a wet-nurse.

But it has been maintained that, besides promoting immorality and crime, the practice of wet nursing is unnecessary, inasmuch as infants deprived of their mother’s breast can be successfully reared upon artificial food. Mrs. Baines says, “I could cite cases where delicate infants who have not thriven upon the ‘hired breast’ have become robust and strong by means of judicious feeding.” Every practitioner probably could do the same, but all we imagine would be able to find a reason for this, other than the general rule that artificial feeding is to be pre-

ferred to a stranger's breast. Conversely, however, there are abundant instances where artificial feeding of various kinds has failed, and recourse has been had at last, and perhaps only just in time, to the "hired breast." She adds, however, "I admit that such instances are rare, from the want of intelligent trained nursemaids. This is the grand difficulty: who is to be trusted to conduct the hand feeding judiciously? and what is the substitute for the breast which is to be preferred? In reply to the first question, Mrs. Baines suggests that young ladies who now overstock the governess market should be trained as nursery directresses; and that, being received into families on a superior footing to an ordinary servant, they should be charged with these important duties—a confession that nothing but a social revolution, such as we can scarcely hope to see effected in our days, will meet the difficulty. With regard to the second question, the replies would be various. Physiology, at least that sort of unquestionable physiology which the late Dr. Latham recommends us to use for practical purposes, would teach us that the proper kind of artificial food would be one which approaches most nearly to the natural food of which the infant has been deprived, and that it ought to contain no alimentary elements of a character different from those contained in human milk. Such a food then we should seek in the milk of some domestic animal. The cow, the ass, and the goat, are the animals to which we should probably direct our view. As respects the ass, we may remark, that the milk is not only too watery, but is remarkably deficient in butter and casein, elements which cannot be supplied. The milk of the goat is exceedingly rich in casein, but is otherwise unobjectionable; so that the suggestion which has been made by Dr. Routh and others, that infants should be put to the animal to suckle, deserves at any rate a fair practical trial, although the milk would not appear suitable for a very young infant, inasmuch as the milk of the human female at the commencement of lactation contains a minimum of casein. For such, cow's milk is better adapted, modified by manipulation in such a manner as to bring the relative proportions of its several ingredients into accord with the age of the infant to be fed. Cow's milk differs from early human milk in containing, at least, twice as much casein and an excess of butter. Hence cow's milk rich in cream being selected, it may be diluted at first with an equal quantity of water, the sugar (very abundant at first in woman's milk) being brought up by the addition of about two drachms of sugar of milk to each pint manipulated. As the infant grows older the milk may be less diluted, and the quantity of added sugar reduced to half the above quantity. In London and large towns where

milk is systematically sold ready diluted, it would be necessary to estimate the dilution undergone by the lactometer, and modify the manipulation accordingly. Mrs. Baines suggests that, by modifying the food of cows, their milk might be made to approximate more closely to human milk, but as yet we are aware of no observations tending to show the practicability of the plan. The teaching of physiology is confirmed by that of experience, which has decided against the employment of farinaceous articles as an addition to the food of young infants. Mrs. Baines regards the professional objection to such additions as the result of prejudice, affirming that the evil results which have been observed to follow their use, arise from bad management, and not from the indigestibility for the infant of the articles themselves. Especially she recommends the addition of wheat flour on the ground of its alkalinity, asserting that, while human milk is alkaline, cow's milk has an acid reaction. Probably she would find, if she made the experiment, that cow's milk, like human milk, is alkaline when fresh drawn, only becoming acid as it stands by conversion of the sugar. The proper precaution would be, not to add a substance foreign to the infant's stomach, but to use milk freshly drawn from the cow, or preserved for use in as cold and clean a place as possible.

Professor Liebig, viewing the question as a chemist, has introduced to the public a "food for infants" as "a complete substitute for that provided by nature." The directions he gives for its preparation are as follow:—"Place in a small saucepan half an ounce of wheat flour, to which add by degrees, and continually stirring, five ounces of (skimmed) cow's milk. Let it boil for about three or four minutes, after which remove it from the fire. Now weigh one part (half an ounce) of malt flour, mix it carefully with thirty drops of solution of carbonate of potash (containing 11 per cent. of carbonate), together with two parts (one ounce) of water. Add this mixture—continually stirring—to the milk pap; after which cover the vessel to avoid its getting cold, and let it stand for one hour and a half. After the addition of the malt flour the vessel should be put into hot, almost boiling water, or near the fire, to keep the mixture warm, which hereby gets thinner and sweeter. After this the whole is again put on the fire to boil up once more, and now the food is to be strained through a muslin or fine hair sieve." It seems to be generally admitted by the panegyrists of this food that it requires to be diluted for the newly born baby. The principle on which its preparation mainly depends is, "the transformation of starch flour into sugar, and the supply of the failing alkali in the milk."

Baron Liebig has stated that thousands of infants of

the Teutonic race have during the last two years been fed with his preparation, and have done marvellously well upon it. M. Poggiale in a communication made to the Académie de Médecine, has denied its similar applicability to the Latin race as tested by experience. He objects to it mainly on the following grounds, namely, because it differs from woman's milk and that of the cow both in physical properties and composition; because it is a chemical preparation demanding the performance of many careful and skilful operations; because M. Liebig has replaced the fatty matter of the milk by glucose, which does not produce the same physiological effects in the economy; and because it is a coarse imitation of natural milk based upon only one and that a very questionable analysis. He prefers cow's milk in the default of maternal milk. After all, we imagine the point to be determined is a practical one, and as yet we are acquainted with no published observations showing accurately its value as a substitute for the milk of the mother for very young infants. In one instance related by Dr. Hecker in the appendix to Liebig's pamphlet, it was found necessary to fall back from this food to the use of a wet nurse, when the infant who had dwindled away upon the former quickly recovered. Certainly we do not think it is yet satisfactorily proved that any kind of artificial feeding is equal in safety, for a young infant deprived of its mother's milk, to the use of a wet-nurse.

We may now pass on to inquire into the relation of illegitimacy to our high infant mortality. And here we are met with the primary difficulty that we have no record of the number of illegitimate infants born (whether dead or alive), and no distinction in the mortuary tables issued from the general register office between the deaths of legitimate and illegitimate children. Where a mother wishes to conceal her fault, and most mothers pregnant out of wedlock may be regarded as thus situated, there is no law to compel her to register the birth, the law only requires that there shall be no concealment of it. Hence our birth-registers probably represent only a fraction of the illegitimate births that actually occur; and when we are told, authoritatively, that illegitimacy is on the decrease in this country, we may accept the statement as only so far true that it represents the reduced number of illegitimate births registered. The fact may merely mean that the mothers of such infants shrink more than they used to do from the proclamation of their shame. "It is," writes Dr. Greenhow, "an unquestioned fact that the rate of illegitimacy of birth is sometimes very large, where the death rate of young children is high; but there is at present no satisfactory proof that these stand to each other in the relation of

cause and effect." The registers, for example, during the ten years 1850 to 1859 show that, in Nottingham, with a ratio of 10·04 per cent. of illegitimate births, the annual average deaths per 1000 of infants under one year was 222·6, while at Wolverhampton the average of similar deaths was as high as 203·3, although the illegitimate birth ratio was only 5·65 per cent. of all births. Turning then to our judicial and criminal annals, we ask what they can show us upon this subject. Mr. Acton writes thus :

"In the year 1867, 1153 inquests were held upon illegitimate children, aged one year and under, against 2960 inquests held upon legitimate children of like age ; so that though the proportion of legitimate to illegitimate children exceeds 16 to 1, the number of inquests held upon the former are little more than double the number of those held upon the latter, thus presenting a ratio of inquests held on illegitimate children eight times as high as it ought to be, taking as our mean the number of the deaths occurring among the legitimate A little further examination of the figures at our disposal seems to place almost beyond the possibility of doubt the supposition, that a large proportion of the illegitimate children brought into the world meet with a violent, or at least premature end. We find that the number of inquests held on illegitimate [legitimate?] children of the age of one year, but under seven, amounted to 2960, while the number held upon legitimate [illegitimate?] children of a like age reaches only to 201. There is here a preponderance of illegitimate deaths striking enough to call forth suspicions of foul play ; but, as compared with the discrepancy exhibited by the earlier period, the proportion has fallen off from one half to one fourteenth. Clearly the time of the strongest temptation to tampering with the life of the infant is during the earliest stage of existence," p. 284.

Dr. Lankester argues much in the same way. In his sixth annual report he writes thus :

"In my own district I find that inquests held on illegitimate children are nearly half those held on legitimate children. When it is remembered that only one child in seventeen is born illegitimate in England, it will at once be seen how large a proportion of these children die under circumstances that demand inquiry. About the same proportion of inquests is held throughout the country, as I find, by the judicial statistics of 1866 that there were 2844 inquests held on legitimate children, whilst there were 1349 held on illegitimate children. Another curious fact connected with these statistics is the disproportion of inquests held on illegitimate and legitimate children between the ages of one and seven years. In my own district, in 1866, there were only eight inquests on illegitimate children between these ages, whilst on legitimate children there were 160 inquests. In the same year, in all England and Wales, there were 190 inquests on illegitimate children, whilst there were 2649 on legitimate

children. This drives us to the conclusion that the illegitimate children are either better treated between one and nine, which is very improbable, or that the large number of them are killed before they reach the age of seven years."—P. 26.

We are not disposed to place the full reliance on such statistics which Mr. Acton and Dr. Lankester do, for two reasons, the first being the uncertainty which attends the enumeration of the illegitimate births in the country, and the second that the proportion of inquests held upon legitimate and illegitimate infants cannot be held to exhibit the relative mortality of the two classes. Inquests are only held where there is something to lead to a belief that an infant may not have had fair play. Now, with legitimate infants it is natural that this belief should be less likely to arise than in the case of the illegitimate, although the circumstances of the two may be in other respects similar. At the same time this line of argument is not without its value, especially when it is kept in mind that suspected and convicted criminals form but a small proportion of the actual existant criminals in any population. It is highly probable that, as all the illegitimate births are not registered, so nothing near the full number of unnatural deaths of illegitimate children come to light, either by coroners' inquisitions or otherwise.

Illegitimate infants are not necessarily, or even usually, put out of the way directly; more commonly indirect means are employed. Still the statistics of the general register office tell us that, in the five years 1863-7, out of the 8398 violent deaths of infants under 1 year of age, 874 were proved to be instances of direct infanticide; and of these 810 infants were murdered within a month of the time of their birth. Of these we find that 218 were strangled, or otherwise suffocated, 59 were killed by blows fracturing the skull by cutting or by stabbing, 51 by intentional neglect, 22 by drowning, 9 by exposure to cold, and 7 by the simple proceeding of leaving the cord untied at birth. The total number of violent deaths of infants under one month of age during the five years, was 3720; the most frequent causes of death being stated to be suffocation in different ways, 1429, navel hæmorrhage 559, neglect or exposure to cold 342, opium in some shape 55, the remainder being burns or scalds.

The deaths registered as infanticide cannot be regarded as including all the deaths thus occurring, even where the verdict of a coroner's jury has exonerated the mother, or those in care of the child, for a reason given by Dr. Lankester, namely, the inconsistent manner in which verdicts of wilful murder are returned in different districts. After showing from the judicial statistics of 1866 how irregular these verdicts are in different

parts, both of the country and of the metropolis, he goes on to say, speaking of Middlesex :

“ It will be seen that just in proportion to the number of verdicts of ‘ wilful murder ’ on newly born children is the smallness of the number of verdicts of ‘ found dead.’ The ‘ judicial statistics ’ do not give the ages of the persons on whom verdicts of ‘ found dead ’ have been held. At the same time, I have no hesitation in expressing my conviction that in by far the larger proportion of these verdicts the inquests were held on the bodies of newly born children. It is the same in other counties. I take the large county of Lancashire, with its teeming population of Manchester and Liverpool. In the whole of that county, in 1866, there were but twelve verdicts of ‘ wilful murder ’ on children under one year of age ; but when we turn to the verdicts of ‘ found dead,’ we find them amount to 506. This is a larger number in proportion to the population than in London itself, and it is the same in other of our manufacturing and thickly populated counties. It is very clear, therefore, that the verdicts of ‘ wilful murder ’ returned by coroner’s courts is no index at all to the amount of infanticide that exists in the whole country. . . . There is, however, still one method left by which we may arrive at a conclusion as to whether the infanticide which is so pronounced in the central and western districts of the metropolis is common throughout England and Wales. I have shown that when verdicts of ‘ wilful murder ’ are not returned, there is reason to believe, from the numbers of cases ‘ found dead,’ that this crime is really involved. The gross number of cases in which verdicts of found dead are returned, as supplied by the judicial statistics for England and Wales, is 2700. Now, I have shown that, in a large majority of cases, these must be instances of children found dead and exposed. In these cases there is, no doubt, a certain percentage due to the discovery of children above one year of age, and to adult persons ; but from my own experience these cases are very few. Taking the year 1866, in which my own returns of ‘ found dead ’ amounted to nine cases, I find that only six of these cases were of persons over five years of age. . . . I think, then, that I have a right to conclude that at least 2500 children are annually found in England and Wales, exposed to the grave suspicion of having been murdered by their parents. . . . There can be no doubt that the origin of this crime is the existence of immorality between the sexes. In these cases it is not a question of prostitution. It is not the prostitute who is the mother of the murdered child, but a woman who has been drawn from the paths of virtue by a seducer ” (6th Annual Report, p. 30).

We think this is fair argument, and that the estimate of the coroner for Central Middlesex is a probable minimum of direct infanticide ; but we have a good deal to add to this if we would arrive at the total of annual untimely deaths of infants due to illegitimate connections. We have to trace the woman and her

infant a little further in life, and see what becomes of the former, and how she disposes of her offspring, when she does not directly kill it. The mother, deserted or neglected by her relatives and lover, must live, and to do this usually only three courses are open to her, burthened, as she is, with an infant. Not one man in fifty, we are told by Dr. Lankester, contributes anything to the support of his bastard child; the utmost which the mother can claim by law is 2s. 6d. per week; and for this there are few women who will put the law in force. Hence, she must either obtain employment at home, or she must have recourse to prostitution, or she must put away her child to nurse, in order to be free for entering upon domestic service as a wet-nurse or otherwise.

The scanty wages obtainable by needlework, to which most women have recourse, almost necessitate their being supplemented by prostitution: and in this way the infant may be preserved from losing its natural nutriment and mother's care; but in any other case, the child must either be disposed of by casting away or by being relegated to the care of a stranger to be brought up by hand. Where one woman takes charge thus of a number of nurselings, the term "baby-farming" has been applied of late years. Most of the infants found in baby-farms are illegitimate. Mr. Acton thus describes the system:

"By it we understand the taking off the parents' hands of infants, by midwives and others, for a consideration. This consideration may consist of payment, made either in a lump sum or by instalments. The number of children that a woman may have under her care at any one time is unlimited. The receiver need not possess any qualification requisite for the due performance of the duties that she undertakes; she need take out no license, and is subject to no inspection or supervision. The women who adopt the calling are, as a rule, of unscrupulous character, and inhabit low and unhealthy neighbourhoods. The facilities afforded by such a system, for the disposal of infant encumbrances, are at once apparent. The chances that the infants will receive fair play at the hands of their strange foster-mothers are small indeed, when we consider that their gains are large in proportion to the number of children received, the shortness of the period over which they survive, and the amount expended on them during that period. It is difficult to ascertain with accuracy the number of the children in this country annually placed in the hands of baby-farmers; if I put it at 30,000, I shall, probably, be under the mark. Even at this computation, it is frightful to contemplate the waste of life and the misery that is going on year by year in our midst, without any serious effort being made to provide a remedy" (p. 281).

These baby-farmers are in communication with another class

of women, who receive into their houses systematically, and as a matter of trade, women who, being in trouble, desire to be clandestinely delivered. It is these women whose advertisements are alluded to at the commencement of our article. By the women who conduct these establishments, some of them situated in respectable streets in London and its suburbs, a complete baby trade is carried on. They take the baby off the hands of their clients on payment of a lump sum, and have various modes of disposing of it to profit. Sometimes a childless couple will be found to adopt the infant in good faith, and will pay for the privilege. Sometimes it is a childless wife, who, hoping to gratify while deceiving her husband, or with other motives, will go through the farce of a mock confinement, and present the bastard as her own legitimate offspring. In such cases the infant is well off; but if no such chance should turn up, the woman who keeps the house has other resources. If sufficiently paid for her risk, the baby will unfortunately be born dead, or if living will be handed over, with a portion of the gains, to the baby-farmer, on the understanding that no one shall be troubled with the infant again.

Other practices are carried on at some of these houses which the law regards severely. In one of the houses described in the papers before referred to in the '*British Medical Journal*,' abortion might be obtained on the payment of a considerable sum of money. To make matters worse, this house was regularly attended by a practitioner in public repute, who, probably without his sanction, was actually referred to in proof that the operation would be conducted with safety to the applicant. Houses of this kind abound in London, and are excused as necessities.

The writer of the paper referred to gives a graphic description of one baby-farm out of many which he visited ('*Brit. Med. Journ.*,' March 28th, 1868). On inspecting the death registers he found that no less than seven deaths of infants, under the care of the woman who kept this farm, had happened in the course of two years. They were as follows:—An infant, aged 11 days, legitimate, of "jaundice;" an infant, aged 14 days, illegitimate, "want of breast-milk;" an infant, aged 3 months, illegitimate "marasmus;" an infant, aged 6 months, illegitimate, "diarrhœa;" an infant, aged 6 months, registered as the son of a baronet, but born in a poor and low street, "congestion of the lungs;" an infant, aged 10 months, illegitimate, "convulsions;" and an infant, aged 12 months, illegitimate, "dentition and convulsions." In the course of our own duties we recently had occasion to visit a similar place ourselves, where not only infants were taken in from the birth, but where very young children were taken, at

the same time, for day-nursing. It was a wretched, ill-lighted, and ill-ventilated garret, in the suburbs of London, where infants and young children, to the number of four or six, and aged from a few days to three years, spent the whole day, and the babies the night also, in charge during the day of a young girl, who had the feeding and care of them. The elder infants were commonly brought at about seven or eight o'clock in the morning, and taken away from nine to eleven o'clock at night. A more miserable set of children we have never seen together. To this house two infants were brought about eighteen months ago from an establishment for the reception and care of the infants of fallen women, and after a few days they were attacked with diarrhœa. They were removed, and we did not hear what became of them. About twelve months ago a female brought there an infant a few hours after birth, stating that it was her daughter's child. In about eight days it was taken ill and died, and the body was taken away by the same woman that brought it. Another infant, the illegitimate child of a servant in a respectable family, by the son of the lady of the house, was brought when about two weeks old; it was soon taken ill, carried backwards and forwards to a doctor at some distance, and died within three weeks, being buried at the expense of the family. Each death in such a house of such a child is doubtless regarded as a "happy release" for all parties concerned. There is always consolation in the thought that the little dear has been taken to a "better place." As to "still-born" children, or those who die fairly or foully within a day or two of birth, undertakers can at any time be found to smuggle them away, giving them Christian burial, and asking no inconvenient questions; but still, of course, "for a consideration."

If, then, we do not know from our national statistics all that we may desire, and probably ought to be able to learn of the influence of illegitimacy upon the unnecessary deaths of our young infants, we still know enough to assure us that it is closely associated with a large amount of crime in the direct or indirect killing of the offspring of unlawful connections, and certainly with very much more than ever comes to light. Were we asked what remedies are applicable to this state of affairs, we should be disposed to say that they should consist in the abolition of the present opportunities of concealment, in an amendment of our bastardy laws, and in a modification in the punishment awarded by the law to the crime of infanticide.

With a view to removing opportunity for concealment, we consider, in the first place, that the registration of every birth that takes place should be rendered compulsory by law, and this whether the infant be born prematurely or at full period, alive

or dead. The registration of births is compulsory in Scotland, and Ireland; and we see no good reason why England and Wales should be exempt from the operation of so salutary a law. Next it is to be observed that, as things stand at present, a registration of death can be made without any other cause being assigned than that which the person effecting the registration chooses to give. In some cases persons keeping chemists' shops have presumed to sign certificates of the causes of death, and medical men have been found who would do so without having seen the subject, either alive or dead, or only when dying, and under circumstances precluding a proper diagnosis. To obviate this loose procedure it should be made necessary to produce, at the time of registration, a certificate from a properly qualified practitioner, that he has had proper opportunities for forming an opinion, and specifying the cause of the death. Under other circumstances, as when death occurs suddenly, or without medical attendance, either the coroner or some officer of the sanitary authority of the district should be required to investigate the circumstances. Six years ago the Metropolitan Association of Health officers represented their views upon this subject to the Registrar-General, urging the necessity of these changes in the law, but nothing came of the communication, as that officer found a difficulty in concurring with the views of the association, and especially with the proposal of registering stillbirths. Thirdly, we think that some statutory check should be placed upon the practice so common among undertakers of disposing surreptitiously of the bodies of infants dying unbaptized and unregistered, in the coffins of other persons or otherwise, and also upon the loose manner in which the bodies of persons are interred in some cemeteries without the production of the registrar's certificate. We have known of instances where this has been kept back by an undertaker upon one pretence or another, thus enabling him to make use of it for the burial of another person that there might be some reason for not registering.

We are not prepared to say that houses to which females moving in the better classes of society, who so far forget themselves as to become pregnant, may retire during the period of their confinement, ought to be mercilessly suppressed. There may be, and probably are, good reasons for the assertion that their existence is a necessity of the times; but they should be put under some sort of authoritative supervision; they should be known to the authorities of the place, and for this purpose registered and licensed, and should be continually kept under the eye of a judicious medical officer. The authorities should grant a license to no person whose character will not bear a full investigation, and the medical attendant of the house and the nurses

should be known and approved of. The same may be said of "baby-farms."

Mr. Acton, in his book on prostitution, dwells at length upon the necessity that there is for an alteration in the bastardy laws, if a check is to be placed upon the wholesale destruction, directly or indirectly, of illegitimate infants. Starting from the proposition that the bastardy law of 1834 is founded upon the mistake that women are the best protectors of their own honour, a proposition to which every man in good position "gives the lie every day of his life, his anxious thought being how best to guard his own daughters from the pollutions which surround even them;" and from the fact that while the weak side of woman's nature, her vanity, love of approbation, and self-sacrificing affection, are taken advantage of by the seducer, whose only motive is the satisfaction of his gross passions, he argues that, as the law stands, the stronger and active party to the sin is permitted to escape, while the whole burthen of the result is thrown upon the weaker and passive sex. She can herself sustain no action for seduction, her only remedy being that she may apply to a bench of magistrates for a summons, obtaining an order in bastardy, entitling her to a maximum sum of half a crown a week for the support of the child up to thirteen years of age. If she declines to make this application, no one can compel her to do so, nor can parish authorities, to whom she and her infant may become chargeable, do it in her behalf. Mr. Acton's remedy is this:

"I would exact, by the help of society, that, in the first place, the seduction of a female, properly proved, should involve the male in a heavy pecuniary fine, according to his position, not at all by way of punishment, but to strengthen, by the very firm abutment of the breeches-pocket both him and his good resolutions against the temptations and force of designing woman. I would not offer the latter, as I foresee will be instantaneously objected, this bounty upon sinfulness—this incentive to be a seducer; but, on the contrary, the money should be due to the community, and recoverable in the County Court or Superior Court, at the suit of the Government Board, and should be invested by the treasurer of such court or by the county, or by some public trustee in bastardy, for the benefit of the mother and child. The child's portion of the fund should be retained by such public officer until the risk of its becoming chargeable to the community, *quasi* bastard, should be removed by the mother's marriage or otherwise, and the mother's share should be for her benefit as an emigration fund or marriage portion" (p. 275).

Holding, as we believe it must generally be admitted, that the intentions of the founder of the Foundling Hospital in London

have been entirely departed from, in the modern management of the charity, Mr. Acton submits that Parliament ought "to interfere, and to appropriate the funds on the *cy-près* doctrine to such object analogous to the founder's intention as shall produce to society the greatest amount of benefit," namely, by setting on foot an institution having for its end objects such as those for whose promotion that charity was endowed :

"I propose the establishment of a Government Board or other competent authority, to whom application may be made in cases of pregnancy by the woman herself or her parents or master or mistress, or other responsible person, whose duty it should be to investigate the truth of the statements on which such application may be founded, and if satisfied of their truth, to take charge of the pregnant woman thrown out of a situation, affording her work and assistance until confinement and during her lying-in. It should further be the duty of the board to settle, at the time of the application, the question of paternity ; and if the seducer have left the neighbourhood, to discover and fix upon him the duty of providing for the child when born" (p. 287).

He proposes to apply the funds accruing for the help and education of illegitimate children, and that subsidiary institutions should be provided throughout the country. He believes that, were mothers thus relieved, a large number would be rescued from their position by marriage with their first paramours, when the latter were persons in the same rank of life with themselves ; for others situations might be found as wet-nurses, and that illegitimacy might certainly be checked, and its sad consequences much softened to the unhappy mother. Furthermore, he says—

"I propose that the power of granting licenses and exercising supervision over all baby-farmers should be vested in this board. All persons wishing to adopt the calling might be required to send in their name and testimonials to it, and to receive a license from it on showing themselves to be fit and proper persons to receive one ; such license should be renewed annually, and a small payment might be made, as duty thereon, by the recipients. The names of all persons licensed might be registered ; the number of babies to be received should be limited, and the receivers should be required to make a return to the board of all infants confided to them, with such particulars as it should seem from time to time necessary to require ; and their houses should be at all times open to inspection by the officers of the board. It should also be made illegal for persons to adopt any children without first satisfying the board as to their ability to maintain them when adopted" (p. 288).

We have made these copious extracts from Mr. Acton's book, not because we see the remotest chance of his suggestions being

adopted in this generation, but because it is apparent that he has given much close thought to a very difficult and complicated subject, and has sketched out a scheme which, if not practicable as yet, is, notwithstanding, consistent.

We shall say very little upon our last remedy, namely, the modification that is necessary in the punishment attached by the law to infanticide. Regarded as murder, and undistinguished from other murders, jurors revolt from convicting in the most flagrant and obvious cases; and the punishment of death seems unmistakably to secure the offender even from the stigma of a murderess, and only serves to ensure her acquittal of the capital crime. If the woman be alone, as she, probably, would be during the act, the jury would be informed that they must be satisfied that the infant was fully born, or the case falls to the ground. She may, indeed, be prosecuted, under a second indictment, for concealment of birth; but this is an evasion which adds nothing to the majesty of the law. As Dr. Lankester says—

“She may conceal her pregnancy, murder her offspring, and if she does not conceal the dead body of her child no punishment awaits her. Within my knowledge, women have barbarously murdered their offspring by strangling them, drowning them and stabbing them, have confessed this crime, and when placed before a criminal court have been acquitted. This has, undoubtedly, arisen from the sympathy of the judge and jury with the women who have been the subject of outrage, and who have been betrayed into crime which forfeits their lives by others who do not share in their condemnation. The law of England, by which capital punishment is inflicted on women who destroy their offspring, is felt to be so severe that judge and jury alike shut their eyes to the facts of the case, and wilfully excuse those who have subjected themselves to its penalties. It is needless to dwell on the immoral effect of the existence of a law which is never carried into practice” (6th Report, p. 33).

However these questions of bastardy, infanticide, and baby-farming, may ultimately be dealt with, it is quite evident that the time has fully arrived when they should receive the thoughtful consideration of the Imperial Parliament. It is nearly forty years since our present bastardy law was enacted; and during the intervening period, questions of social importance have risen to the surface and received an amount of attention from all classes of the educated community, at any rate equal to that which has been bestowed upon any matter of general political interest. Public sentiment upon many of these questions has been greatly modified concurrently with the advance of public information.

Neither can we refuse to see that the educational and religious

efforts of the last thirty or forty years have resulted in a higher sense of moral obligation. We perceive it manifested on all sides at public gatherings for political as well as social objects. We see it in the changes which have been made in the laws of bankruptcy, in our factory acts, in the attempts to legislate against fraudulent adulterations of food and articles of commerce, in the tone adopted by candidates upon the hustings, and by ministers of the crown in parliament. We cannot believe that the whole of this is hypocritical. There is a growing feeling that right should prevail over expediency and might. On this feeling we depend, to this we believe we may firmly trust for many much-needed social reforms.

E. BALLARD.

VIII.—Wharton Jones on Failure of Sight.¹

MR. JONES divides his work into three parts, of which the first treats of the pathology of the amaurotic failure of sight caused by railway and other injuries of the spinal cord; the second, of the pathology of the same condition, caused by similar injuries of the brain; whilst the third part treats of its diagnosis, prognosis, and treatment. A very large portion of the first part is occupied with the consideration of various physiological facts connected with the circulation of the blood, of the phenomena attendant upon the inflammatory process, and of the physiology of the action of the iris, with the influence upon it of mydriatics and myotics—a portion so large, indeed, that it may be doubted whether it does not exceed in length, as it certainly does in interest, that which is devoted to the proper subject of the work.

It will be observed that in the clinical section of the treatise the topics embraced by Mr. Jones almost exclusively relate to the intra-spinal and intra-cranial conditions leading to amaurosis; and we find little or no remark made upon direct lesions of the organ itself, such as detachment of the retina and cataract, though we have seen more than one case where the latter affection has been believed to be caused or its progress hastened by a violent concussion of the head. Mr. Jones's object is, therefore, to show the symptoms, diagnosis, and treatment of amaurosis resulting from concussion or other affection of the central parts of the nervous system.

¹ *Failure of Sight from Railway and other Injuries of the Spine and Head, its Nature and Treatment, with a Physiological and Pathological disquisition into the influence of the Vasomotor Nerves on the Circulation of the Blood in the Extreme Vessels.* By THOMAS WHARTON JONES, F.R.S., F.R.C.S., &c. London, pp. 309. 1869.

It seems to us to be questionable how far it is advisable that a book should be written on the effects of one particular form of injury, since if this mode of dealing with such subjects be adopted generally, we shall next be having a special treatise on loss and defects of smell after railway injuries, or on what we have so frequently heard alleged of late years, and which no mortal can disprove, loss of virile power, and it appears to us just as legitimately. We much wish, indeed, that Mr. Jones had discarded the word "railway," instead of putting it into prominent type. It looks too much like touting for what is known to be a lucrative branch of practice; and though Mr. Jones's position and scientific attainments place him far above any suspicion of the kind, yet it paves the way for others to come forward who may be less scrupulous. Another objection to this proceeding appears to us to be that it leads to the consideration and treatment of one or two symptoms, and not of the affection as a whole.

We would ask Mr. Jones whether, in the cases he describes, accompanied by lassitude, weariness, and inability for mental and physical exertion; with the patient suffering from pains, tinglings, and numbness of the limbs; from fixed pain and rigidity of the spine; from mental confusion and signs of cerebral disturbances; and, finally, from loss of motor power and peculiarity of gait—whether the presence of symptoms connected with the organs of the sense would, in any one point, cause a modification of the line of treatment that would be carried out by any intelligent practitioner for the general effects of the shock or injury? We think not. We should very much, therefore, have preferred to have seen a general treatise on shocks to the nervous system from Mr. Jones's pen, and in this we are sure nearly all the general observations he has here made might have been appropriately inserted. When we turn to the description of the visual affections given by Mr. Jones they appear to us, in many points of view, most unsatisfactory; for, in the first place, it is to be observed, that in many of these cases, the symptoms complained of are purely subjective, and that they are obtained under circumstances of all others most favorable to deception. Take the first case recorded by Mr. Erichsen. We learn that, "on examining the state of the eyes, Mr. Erichsen found that vision was good in the right eye, but that this organ was over-sensitive to light. In the left eye vision was nearly lost, so much so that Mr. R. could not read large print with it." These statements may, of course, have been facts, and we do not wish to dispute them, but what evidence was obtained as to their truth? Did Mr. Erichsen test the veracity of the patient, and if so, in what manner? To do so properly and satisfactorily requires a skilled

observer in ophthalmic practice. For ourselves we know of few assertions it is easier to make and harder to disprove than that the sight is failing. Unless the patients complain of it themselves, absolutely without having a single leading question put to them, and there is some alteration in the size or activity of the pupil, or some recognisable structural change in the fundus, we greatly distrust all accounts of sparks, and coloured rings, and muscæ, and so forth. In our own recent experience we have twice been able to satisfy ourselves that the statements of patients endeavouring to obtain compensation from railway companies were untrue. In one of them, amongst other symptoms, intolerance of light was complained of, and the light of a small lamp, raised for the purpose of examining the eye, could not be borne for a moment; yet, whilst the patient was otherwise engaged, the light of two such lamps could be sustained in immediate proximity to the eye, and we subsequently obtained a fairly steady ophthalmoscopic examination. In the second case the patient declared he could not see C of Snellen at ten or twelve feet, although by careful application of prisms and of the stereoscopic test his untruthfulness was clearly shown.

It is a point to be borne in mind, too, that we rarely see the end of these cases. They are watched and treated to the time of trial; they obtain their compensation, and then we hear no more of them. Railway collisions have been tolerably numerous during the last fifteen or twenty years, and those who suffer most are notoriously the occupants of the third-class carriages. Yet how many cases of amaurosis, caused by railway injury, appear in the out-patient rooms of our ophthalmic hospitals. No! those who complain of these ophthalmic symptoms are generally the well-to-do man who can afford the daily visit of his attendant, and who learns from him the symptoms from which he is expected to suffer.

Take Mr. Erichsen's third case quoted by Mr. Jones. This patient suffered from impaired sight, with muscæ volitantes, sparks and flashes of light before his eye, inability to read beyond a few minutes, partly because the letters seemed to run into each other, and partly because he could not concentrate his thoughts so as to fix his attention; and eight years after these symptoms were still present, though in a less severe degree! Here, again, we must object that all the symptoms were here subjective, and that we have no account whatever of any ophthalmoscopic examination, though we can scarcely imagine such symptoms to continue for so long a period without revealing themselves by some physical change, recognisable under the ophthalmoscope.

In the same way with many of the cases Mr. Wharton Jones

records, the symptoms are chiefly subjective, whilst the ophthalmoscopic appearances are slight and unimportant. In Case VI, for example, in which the patient had had a fall, and suffered from the subjective symptom of dimness of vision and sparks with pain in the right eye, the ophthalmoscopic appearances were "a bluish whiteness of the optic disk on the apparent inner side, with congestion and blackish discoloration of the retina, all round, from granular pigmentous deposit." Now these are by no means uncommon appearances, and granting them to be significant, *per se*, they lose at least half their value when we are not told the condition of the left or sound eye, nor possess any information whatever in respect to the condition of the eye antecedent to the time of the accident. So, again, Case VII appears to us to be a simple case of physiological excavation of the optic disk, which may be concurrent with perfect vision. In Case VIII the ophthalmoscope revealed a general fulness of the vessels of the fundus; but this, it is well known, varies with a number of conditions which require consideration, while it is subsequently stated that seven months after the previous examination the optic discs were the seat of capillary injection, and presented in their temporal halves glimmerings of a bluish whiteness, a statement that we are at a loss to understand. There can be little question that any such injury affecting the upper part of the spinal or cilio-spinal region may through irritation of the sympathetic nerve on the vessels influence the supply of blood to the retina the condition of the pupil, and seriously interfere with vision, but we are not prepared to admit that the changes proved are so clearly recognisable with the ophthalmoscope as to permit statements to be made on oath, or which would justify the practitioner as the title of Mr. Jones's book would imply in dissociating the local effect from a general nervous affection.

In that part of the work which treats of the diagnosis, prognosis, and treatment in these cases, Mr. Jones himself points out that the diagnosis must frequently be doubtful, especially as, in some instances, there may be failure of sight, with little or no appearance of anything abnormal under the ophthalmoscope. It is obvious that we must here rely entirely upon the patient's statements, a proceeding that no astute counsel will allow to pass without comment.

As regards the prognosis he regards it as generally unfavorable, especially when the amaurosis proceeds from concussion of the spine rather than from concussion of the brain.

The chapter on treatment contains several points of interest. Mr. Jones shows that the general symptoms, as well as the effects produced by belladonna, under the microscope, show that

this drug occasions contraction of the small arteries. In this it agrees with ergot of rye, and hence the value which has long been empirically recognised of these two agents—

“In spinal congestion, meningitis, or myelitis, in which there are symptoms of irritation; while, on the contrary, it is calculated only to increase the paralysis in cases of white or non-inflammatory softening of the spinal cord, which is characterised by the absence of symptoms of irritation. Hence,” he continues, “from my experience of belladonna, it results that, in cases of internal venous congestion, like that which is of such frequent occurrence in persons afflicted with glaucoma, belladonna would operate prejudicially.”

On the other hand, the antagonistic action between belladonna and Calabar bean, which has been shown to exist by Dr. Fraser, would lead us to anticipate that the latter is likely to prove of use in those cases of intraspinal disease to which belladonna has been found by experience injurious; and as the Calabar bean excites contraction of the muscular coats of the veins, so it is in venous congestion of the spinal cord that the Calabar bean is most likely to prove beneficial. Partly led by these considerations, and partly as a means of simply effecting contraction of the pupil in a state of wide dilatation from paralysis of the head, Mr. Jones applied a solution of Calabar bean to the eyes in a case of ptosis and mydriasis; and from the rapid acquirement of power by the patient over the paralysed muscles, believes he has obtained a remedial means of considerable value in such cases. This result, again, naturally suggested the employment of atropia in cases of paralysis of the external rectus; since “the sympathetic nerve, at the same time that it governs the consensual dilatations of the two pupils, appears to have some influence on the action of the external muscles which is consensual therewith;” and the remedy was employed in one case with justifying success, almost complete recovery occurring in the course of a month.

In the treatment of the eye symptoms which supervene in cases of spinal and cerebral concussion, Mr. Jones states he has found the iodide of mercury, in doses of from one twentieth to one tenth of a grain twice a day beneficial; and after it the iodide or bromide of potassium, with iron, followed up by iron and *nux vomica*.

In reference to cases of embolism of the central artery of the retina, Mr. Jones questions much whether they are really of this nature, or at least wholly so, but considers that the well-known symptoms of pallor of the optic disc, constriction of the arteries, deep colour and increase in size of the veins except at their point of exit, absence of pulsation in the arteries even on

the application of firm pressure to the globe, occasional ecchymosis, and swelling and opacity of the retina in the region of the macula lutea, are all symptoms that do not necessarily point to the presence of an embolon in the central artery. The state of the vessels is, he thinks, merely such as would arise from spasmodic constriction of the artery; "and such constriction is just what might occur suddenly and determine the failure of sight by stoppage of the afflux of arterial blood to the already morbid retina. For it is to be observed that the whiteness of the disc, and swelling and opacity of the retina in the region of the macula lutea, could not have been of the same recent occurrence as the sudden failure of sight. The probability seems to be, that deterioration of the structure of the optic nerve and retina has been gradually taking place in one eye, with corresponding impairment of sight, though as yet undetected by the patient, when the spasmodic constriction of the central artery, supervening suddenly, aggravates the blindness and draws the patient's attention to it. Heart disease has been generally, though not invariably, detected in such cases; but embolism of the vessels of the intracranial part of the optic nervous apparatus would be quite cause enough of the disturbance of the circulation in the eye, without appealing to an embolon in the central artery of the retina itself. If in any of the cases there was really a plugging of the artery, the peccant matter was probably softened puriform fibrin, such as Mr. Gulliver has described in clots of the heart, arteries and veins, and originally discovered by an extensive series of experiments and observation to be a distinct and important elementary form of disease. Long after the publication of Mr. Gulliver's observations, Mr. Jones observes the results were described anew under the name of 'thrombosis.'"

The physiological portion of the work contains many points of interest, as might be expected from one who has so long occupied a prominent position as an experimental physiologist. Mr. Jones expresses his opinions very decidedly in regard to Cohnheim's views on the migratory corpuscles, and considers that he has not correctly interpreted the phenomena he witnessed in the mesentery of the frog under the conditions subject to which his observations were made. Agreeing with Cohnheim in repudiating Virchow's doctrine of proliferation, he has yet been unable to convince himself that any of the colourless corpuscles actually make their way out of the interior of the veins or capillaries, since even when the corpuscles appeared as if accumulated outside the vessels he could always recognise the wall outside by adjusting the focus of the microscope on it; and when at a later stage he was unable to recognise the out-

line of the wall outside the accumulated colourless corpuscles, he was as little successful in recognising an outline of the wall inside, for the wall had by that time given way. Moreover, he thinks that, if the white corpuscles do escape from the blood-vessels, it cannot for a moment be admitted that the walls remain entire. It will be seen, therefore, that Mr. Jones is by no means disposed to accept Cohnheim's statements unreservedly.

We regret that the space at our disposal will not allow us to comment upon the interesting remarks made by Mr. Jones on the action of atropine and the Calabar bean on the pupil, and those having reference to the action of the sympathetic on the circulation in the eye; but for these we must refer our readers to the work itself, the physiological portion of which will well repay perusal.

IX.—Army Medical Department Report.¹

THIS, the ninth, volume fully sustains the reputation of its predecessors in interest and value. The annual Reports of the two services, the Army and Navy, are now justly regarded as among the most important contributions made from year to year to medical literature. From the authentic records of sickness and death in numerous and diverse regions of the world which they present, they give information on many points respecting the history of disease which can be found nowhere else. On this ground alone they well deserve the attention of all scientific physicians.

The main strength of the British army (white troops) in 1867 was 175,752. Of this total, 130,000, in round numbers, were stationed at home or in India; 15,000 in North America, and about 10,000 in the Mediterranean. The admissions during the year were 180,000, and the number constantly on the sick list was a trifle over 8000. The total deaths amounted to 2914; the number of men discharged from the service as invalids was 3361. As compared with the average of the previous seven years, 1860-66, the millesimal sickness and death rates in 1867 stood thus:—1023 to 1166, and 16·58 to 17·04. The invaliding also was lower than it had been, as the ratio of 18·9 per thousand of mean strength does to that of 24·4; and the proportion of men constantly non-effective from sickness was as 45·82 to 51·86. These several data seem, therefore, to indicate

¹ *Army Medical Department Report for the Year 1867.* Presented to Parliament by Command of Her Majesty. 8vo, pp. 563. 1869.

a general amelioration in the health-state of our army; and this is so far satisfactory as showing that we are progressing in the right way; for there is still an immense amount of hygienic work to be done before much unnecessary sickness and death are, as they might be, prevented. Among the troops stationed in Great Britain in 1867, there was much less fever, as well as of bowel disorders, than in the previous year, and not a single fatal case of cholera occurred. In Ireland, however, especially in Dublin, the mortality from fever was unusually high, in consequence of an outbreak of 'cerebro-spinal meningitis' attended with purple blotches, or 'purpuric fever,' as it was sometimes called.

In the Mediterranean, an epidemic of mild fever, closely resembling the "break-bone fever" or "dengue" of the West Indies, prevailed during the last quarter of the year at Gibraltar, both among the military and civil population. There was no death among the former. The disease had been prevalent at Cadiz, "from whence it was believed to have been introduced"!

Diarrhoea and dysentery were more frequent in the garrison than in 1866, and considerably in excess of the average of previous years; but no case of spasmodic cholera occurred.

Malta fared much worse. A bad form of fever, often fatal, prevailed among the troops between June and October, and especially in the notoriously unhealthy lower St. Elmo barracks, and also in forts Ricasoli and Salvatore. Bowel fluxes were also in excess, and we read that "the corps in which fevers were most prevalent furnished the largest number of cases;" always a very significant circumstance. But besides ordinary diarrhoea and dysentery, an outbreak of malignant cholera occurred this year in Malta, between July and the end of October. The island had remained free from the pestilence since the latter part of 1865; but now, in spite of quarantine precautions of extraordinary rigour against all suspected arrivals, it again found its way in. It had been present, for some time previous, in the adjacent part of the Barbary coast at Tunis and the neighbourhood. There were twenty-eight fatal cases among the soldiers, and eight among the women and children of the garrison. The sanitary state of much of the barrack accommodation is still very faulty; "When so many prominent causes of sickness really exist" in most of them, says Inspector-General Paynter, "it is difficult to select any one, or even two, as being the most evident." Intemperance is another fruitful cause of illness.

Passing now over to the New World, we have only to remark, in respect of the troops in North America, that neither in 1866

nor in 1867 did any fatal case of spasmodic cholera take place in any of the British provinces, although in both years, specially in the former, the pestilence prevailed in various parts of the United States. The fact is instructive, as showing how much less widely diffused was the morbidic poison during the last epidemic than on some former visitations; and this, too, notwithstanding a greatly increased intercommunication between the two countries.

At Bermuda the noteworthy fact was that, besides the endemic fever being of a decidedly more fatal type than usual, four cases (three fatal) of genuine yellow fever occurred in October. All of them took place among the artillery quartered at St. George's, the locality where the disease has almost always been most prevalent and deadly. The troops were camped out, and no further case occurred. "No satisfactory explanation could be given of the cause of this sudden outbreak of yellow fever." That the cause should have been noxious exhalations from an adjacent cemetery, as the principal medical officer seems to have imagined, is very unlikely. It is much to be regretted that no detailed report has been given of all the circumstances relating to the occurrence of these sporadic cases of a disease respecting which there is still so much discrepancy of opinion.¹

In some of the West India islands this formidable fever was present during the latter half of the year. The first of the four cases, three of which were fatal among the troops at Barbadoes, occurred in an officer who had just arrived from St. Thomas's, where the disease has been of recent years seldom absent. The barracks in Barbadoes, as at Bermuda, are in many respects very faulty. The troops at Guiana fortunately escaped, although "yellow fever was very prevalent among the shipping at Demerara." In the seamen's hospital there, out of 473 cases admitted during the year 111 were fatal. In Jamaica a severe outbreak took place. There is a full and valuable special report, prepared by a military commission, consisting of the principal medical and engineer officers and of the Adjutant-General, and illustrated with maps and diagrams, on this epidemic. The details, particularly those respecting the prevalence of the fever, for the second time, at the mountain cantonment of Newcastle,

¹ In the previous year, 1866, there had been an unusual amount of fever, chiefly typhoid, among the civil population as well as the garrison. The mortality was much above the ordinary rate. Alvine fluxes, too, were doubly more frequent and fatal; outbreaks of yellow fever have often been preceded by the prevalence of these diseases. Moreover, in 1867, the pestilence existed in many of the Southern States of the American Union, and reached to Memphis in lat. 35°, and to Vicksburg, still further to the north, on the Mississippi. Bermuda is in lat 32°. *Vide Epidemiological Memoranda, by Dr. Milroy, in this Journal for July, 1869.*

nearly 4000 feet above the sea-level, are extremely interesting; but we cannot at present enter upon their consideration, or do more than merely state that out of a force of 900 men and officers in the island, there were 100 cases and 36 deaths, exclusive of 11 cases and 5 deaths among 105 women. The black troops escaped; but they seemed to have suffered more severely than usual from continued fevers. Among the white troops, alvine fluxes were considerably above the average.

As in the case of Bermuda, there had been an unusual amount of continued fever, causing an excess of mortality among the troops in the previous year. What adds not a little to the interest of the military report on the Jamaica epidemic in 1867 is the circumstance that an able and very elaborate account of its prevalence in the ships of war and the naval hospital at Port Royal appears in the 'Report of the Navy' for 1867, recently issued.

The health of the troops serving in the Cape of Good Hope colony was, throughout, 1869 fairly good. They suffered but little from the epidemic fever which prevailed to a serious extent in Cape Town, and to a less degree in Port Elizabeth and that part of the colony, and of which an excellent report is given, which will be noticed in the sequel.

The island of Mauritius continued to suffer, and more disastrously than ever, from the malarial endemic which had prevailed throughout nearly the whole of 1866. It raged most severely in April (corresponding with October in north latitude), and then gradually decreased till October. The total death-rate among the civil population of the whole island for the year was estimated at 111 per thousand, of which not less than 83 per thousand was due to the fever. In Port Louis alone, where the disease was most prevalent and fatal, the death-rate was more than twice as great, amounting, it has been said, to 274 per 1000.

Among the troops, as among the civil population, the fever prevailed most in April, when the admissions for the month were in the ratio of 305 per 1000 of the strength. During the first half of the year the number of the garrison averaged 1700; in the latter half a little under 1000, in consequence of many of the troops having been removed to the Cape or sent to England.

In Ceylon the sickness and mortality among the white troops, whose average strength during 1867 was a little over 900, were considerably below the mean of the previous eight years. There was much less alvine flux, the most fruitful cause of inefficiency and loss in this climate, than usual, and only one case of spasmodic cholera occurred; it was fatal. The native troops also

were less sickly than in most years. In both classes of troops the invaliding, too, was much below the average.

The number of our European troops in *India* during 1867 averaged 56,869; the admissions into hospital amounted to 78,437, and the deaths to 1529, to which latter must be added 113 among invalids on the passage home or at Netley. The death-rate was, therefore, 28·86 per thousand of mean strength, being very considerably above that in 1866, but little over the average of the preceding seven years. The excess was confined mainly to the *Bengal* presidency, in which the ratio of mortality was nearly thirty-four per thousand, which is considerably above the average of the last septenniad. This high rate was due to the prevalence of epidemic cholera in the upper and north-west provinces between May and September. As this epidemic is one of unusual interest and import from various causes, it well deserves a special article to itself, and we trust to find space for this in an early number.

In the *Bombay* presidency, the death-rate, of 21·07 per 1000 of mean strength, although considerably higher than in 1866, was under the average of the previous seven years, the reduction being chiefly in respect of miasmatic diseases. The only station where cholera prevailed with epidemic force was at Nusseerabad, in latitude 26° N., nearly due west of Ajmere and Gwalior. It was confined almost entirely to the 2nd battalion, 1st regiment. The first case occurred on September 3rd, and the last on the 29th, and in that short time there were sixty-two cases and forty-nine deaths. Five cases and three deaths occurred in the Royal artillery at Nusseerabad. In neither of the corps were the officers attacked. Many of the women and children of the 1st regiment succumbed. The other troops at the station seem to have escaped, and only sporadic cases of the disease occurred during the year in other parts of the *Bombay* command.

The health of the *Madras* army was fairly satisfactory. The death-rate, 21·40 per 1000, was below the former average. Although more deaths from diarrhoea and dysentery occurred than usual, cholera did not prevail epidemically among the European troops anywhere. There were only three deaths from the disease, one at Bellary and two at Secunderabad. Many of the fatal cases of dysentery too occurred at the latter notably unhealthy station. At Poonamallee also, another bad locality, now about to be abandoned, there was much dysenteric sickness. Of the town of Madras, in which cholera has long been believed to be endemic and firmly established, Inspector-General Hadaway, referring to its exemption during 1867 remarks :—

“It may be hoped that this unusual exemption has not been due solely to fortuitous circumstances, but is, in part, attributable to the judicious sanitary measures now adopted at all the great native festivals, where, amidst the vast concourse of poverty-stricken devotees from every distance and every point of the compass, the disease may be said to have been in former times almost *forced*, and whence its seeds used to be scattered in every direction by the infected streams of returning pilgrims.”

Intemperance was, unhappily, a frequent cause of hospital admissions in the last quarter of the year, when “the larger amount of money received by the men as bounty on re-engagement, as prize money, and as arrears of the increase of pay granted to the army from April 1st, 1867, seems to have been too generally spent in debauchery and dissipation.”

Among our troops serving in China and Japan, the amount of sickness among the white soldiers in *Hongkong* was greatly less than usual—less than in any year since this island has been occupied as a military station—the reduction being most marked in respect of miasmatic as well as of enthetic diseases. This is very gratifying, and we trust that the improvement will be lasting. It is attributed to “the increased space given in barracks, the moderate amount of night duty, good diet, and general attention to sanitary arrangements.” The former history of this colony has been a sad lesson-book of the prodigal waste of human life from official neglect of hygienic principles and requirements. The diminution in venereal cases “has been probably the result of a much more rigid enforcement of a local act which has been in operation for the last ten years.” Among our native troops serving in Hongkong, averaging 425, the sickness was unusually great during the year, chiefly from small-pox, chronic rheumatism, frequently with syphilitic complication, general dropsy (anasarca), and bronchitis. The prevalence of anasarca among one section of the Ceylon rifles is attributed to their faulty diet. “The detachment consisted of two companies of Malays, and two of Tamils, chiefly of the Hindoo religion; the former eat any kind of animal food, the latter live principally upon fish and vegetables.” Almost all the cases and all the deaths occurred among the Tamil troops.

The average strength of the troops serving in *Japan* this year was 773; the sickness and death-rates were less than in 1866. Numerous cases of small-pox, which is endemic in Yokohama, occurred, but happily none were fatal. “It is remarkable that in the only two cases in which the disease presented the confluent form, the men bore not only marks of vaccination, but also of previous small-pox.” Venereal disorders were excessively prevalent; six per cent. of the force being constantly in hospital

from this one cause. To this and to intemperance much of the sickness was unquestionably due.

The average force in our Australasian colonies—*Australia*, *Tasmania*, and *New Zealand*, amounted to 3499; the sickness and death-rate in all of them were very low. No epidemic disease occurred among the men; but scarlatina and measles prevailed among the children of the regiments. Influenza and sore-throat were common. The only noteworthy point is the mention of the frequency of phthisis as a cause of death and of invaliding among the troops in Australia. "The cases, however," Deputy Inspector Gibb remarks, "do not appear to have originated there, but to have arrived in the country with tubercles in their lungs, which, from exposure, intemperance, and other causes, had developed to a fatal termination." He warns the colony that "the introduction of cholera is merely a question of time, as it will some day arrive from the increased facilities of commerce and more rapid communication with other countries."

It is worthy of note that among the large number of troops on board ship during 1867, proceeding on or returning from foreign service, or passing from one colony or military station to another, no epidemic broke out in any of the vessels. No death from spasmodic cholera occurred among the troops returning home from India. The only instances were in the case of a detachment of the 60th regiment proceeding from Calcutta to Madras, when three men died on the voyage; and, curiously enough, three men of another battalion of the same regiment stationed at Malta died in the same month, on their passage from the Mediterranean to Canada. "The battalion embarked at Malta on September 5th, after these diseases (cholera and choleraic diarrhoea) had been prevalent for some time among the civil population, and one fatal case had occurred among the women of the regiment six days before embarkation. During the voyage there were one case of cholera and fourteen cases of diarrhoea among the men. The fatal case of cholera occurred two days after embarkation; and, on the same day, a child was attacked and died after nine hours' illness. Fifteen days after the Himalaya had left Malta, a fatal case of cholera occurred among the ship's company, and diarrhoea is stated to have been prevalent among them."

The Appendix contains, as usual, several highly interesting papers. First on the list is the Code of Instructions by the Army Sanitary Commission for conducting inquiries respecting the outbreak and prevalence of cholera in India. The proposed scheme is calculated, we think, to do a great deal of good for the profitable investigation of epidemic disease in future. Hitherto,

the real knowledge of this important branch of medical study has, notwithstanding the incessant publication of new works, and the accumulation of fresh experience on the subject, made far less sure advance than might reasonably be expected. The reason is obvious. Too generally, the evidence adduced is loose, meager, and insufficient, and the conclusions hasty and illogical. Hence the ever-recurring fluctuations and discrepancies of opinion and the conflicting recommendations urged by medical men upon the public; and, as a necessary consequence of this, the marked unwillingness of governments to be guided by mere professional authorities in matters affecting national and international interests. It seems to have been from the unsatisfactoriness of much of the evidence respecting the recent outbreak of cholera in India that the War Office has felt the necessity of having instituted a more thorough and comprehensive examination of the disease, its properties or attributes, and the laws which govern its development and diffusion, before Government could sanction any fresh enactments and fresh outlays for its subjugation or prevention among our enormous Indian population. Many most useful results may be anticipated from the scheme now laid down. Its adoption should, of course, not be limited to India; obviously it should apply to all our colonies, and, in short, to wherever our troops are stationed, whether abroad or at home. A similar scheme might be adopted with great advantage by the sister service, and we are glad to hear that the new Director-General is favorable to the idea. Few men have better opportunities of determining many of the characters of cholera and of other epidemic diseases than the medical officers of the Navy; and it is therefore of the utmost importance that their investigation of these maladies should be so conducted as to obtain the most exact and trustworthy results possible.

From Dr. Parkes' 'Annual Report on Hygiene' (for 1868), which always contains instructive matter and remarks, we shall only extract what he says as to the chemistry of water in relation to health. "At the present moment matters have been brought to such a pass that, if a question of water analysis came before a court of law, hardly any chemical point could be raised on which a chemist might not be called on either side. This is certainly unfortunate, and shakes the confidence of the public in hygienic water analysis. . . . The chemists have almost universally decided against the incineration plan, and the employment of permanganate of potassium." What is said about "cholera from water-poisoning," and "the outbreak of cholera at Hurdwar," we must reserve for another occasion.

The narrative of the Medical History of the Abyssinian Ex-

pedition, by Inspector-General Dr. Currie, is replete with interesting details. We shall point out a few of the most important. In consequence of the total absence of water, upon disembarkation in Annesley Bay, the whole supply for men and animals, in the general camp on the sandy plain at Zoolla, lat. 15° N., had to be obtained from distilling sea-water. The consumption was necessarily very large, amounting at one time to 50,000 gallons per diem. Each man was allowed a gallon and a half daily, and the animals were allowed to drink from the troughs once a day *ad libitum*. The watering of many thousands of animals¹ daily was an interesting spectacle. In the mountain pass leading up from the plains of Koomaylee, about twelve miles from Zoolla, to the Senafe plateau, situated between seven and eight thousand feet above sea level, the scarcity or almost total want of water was fortunately overcome by the use of Norton's American tubes and Bestien's chain-pump at the several stations established by the troops along this romantic defile. "By these means water of excellent quality was obtained, and after a little while sufficient in quantity to meet every demand."

On the journey thence to Magdala, the water used was almost entirely surface water, drawn from pools or small streams. The expedition was accompanied throughout by 'bheesties' or water-carriers, a valuable class of Indian camp-followers, who supplied the troops; every soldier had a water-flask covered with canvas.

The troops from Bombay began to arrive early in December, 1867; and the whole force, amounting to 12,500 (4,200 Europeans, and 8,300 native troops), had landed by the beginning of the following February. From January to March, troops were continually being moved up from Zoolla to Senafe, and thence on to Antalo, which is about half-way to Magdala. A railway had been laid down from the coast to Koomaylee, and the steep road from that point to the mountain plateau was made, by the great exertions of the troops, practicable for carts as well as for the transport animals. At Senafe a great permanent camp was formed, on an extensive undulating plain, bounded on one side by rocky peaks and precipices, and on the other by an open valley, from which abundance of water was obtained by means of the American tubes. The advanced column reached Antalo early in March, and there a large commissariat dépôt, and dépôt hospitals, were established. Between Antalo and the Takazie river, a distance of about 130 miles, the intervening Alpine

¹ The number of animals landed at Zoolla was as follows:—Elephants, 44; camels, 5738; mules, 16,022; ponies, 1651; donkeys, 1759; and bullocks, 7071.

region is loftier and more difficult for troops than in any other part of Abyssinia. Dr. Currie describes it "as a sea of mountains, entirely volcanic, running irregularly north and south, and forming the watershed—the drainage on the west side running to the Nile, and that on the east to the sea. These mountains are crossed by others of great elevation, extending east and west, and from the summit of some of these passes glorious views were obtained." The passes crossed were 10,000 feet high. Difficulties of another description had to be overcome before reaching Magdala, about 80 miles distant from the last mountain range, viz.: the passage of the deep chasms of the Takazie and two other rivers. The descent of the right bank of the Takazie, one of the principal tributaries of the blue Nile, was tolerably easy; but the ascent of the south side, a precipitous wall of volcanic rock 2700 feet high, was excessively steep and difficult. On the face of this, the pioneers constructed a narrow zigzag bridle path, three miles in length, by which the troops and baggage animals were enabled to ascend to the Wadela plain. This was on the 28th of March. The first encampment here was 10,450 feet above the sea level, and the cold at night was 7° below freezing point. The daily range of the temperature for some time past had been so great that the nights, without much exaggeration, might be described as Siberian and the days tropical. "The long ascents and descents of mountain passes and formidable ravines proved excessively fatiguing, and demanded greater physical exertion and powers of endurance than British troops have probably ever undergone. The effect of this severe training was shown in their increased capability of making long and difficult marches day after day, and with comparative ease to themselves." After crossing the basaltic ravines of the Jeeda and Bashilo rivers, deeper considerably than the ravine of the Takazie, the troops having now marched nearly 400 miles from Zoolla, were within 10 miles of Magdala, before which the advance brigade arrived on April 10th. The short but decisive action which ensued in the course of that day, when Theodore's troops were completely routed, with a loss of 700 killed, besides 1200 wounded, and with only a small number of casualties in our force, which consisted of 1726 Europeans and 1744 Sepoys, determined the fate of the campaign. It was on the 13th that the place was stormed and taken, and the unhappy king slew himself. Five days after, our troops began their retrograde march, and reached Senafe on May 24th. On coming to the bottom of the pass, they were conveyed by railway straight on board the transports ready to receive them; and the embarkation was so expeditiously effected, that almost all the troops were away by the first week in June. Zoolla was finally

evacuated on the 19th of that month. The heat in the plains had been for some time intense; the thermometer in tents at Koomaylee standing at 114° , and at Zoolla at 106° .

The health of the expeditionary force during the twenty-five weeks they were employed in Abyssinia was, on the whole, very satisfactory. From the absence of intemperance and the small amount of venereal disease, much sickness was avoided. Excellent hygienic and sanitary arrangements had been made, and they were efficiently carried into effect by the medical staff under the able direction and guidance of Dr. Currie. The prevailing diseases were bowel complaints and fevers, chiefly intermittents. To the former rather more than a fourth of all the admissions was due, and to the latter nearly a fifth. What proportion of the ague cases originated in Abyssinia, or were relapses of the disease contracted in India, it was impossible to determine. Of the 46 deaths among the European troops, men and officers, 19 were due to dysentery, and only 5 to fevers. Eight died from sunstroke; these were, of course, in the plains. Three were caused by gunshot wounds; but of these 2 were, unhappily, cases of suicide, and 1 of accident. Of the total deaths, 22 occurred in the lowlands, and 24 in the highlands. As to the Native troops, there was a remarkable concurrence in the prevailing diseases and in the extent of sickness and mortality between them and the European troops. Both suffered nearly in the same proportion from bowel complaints, the prevalence of which "was attributable to a combination of causes, the more efficient of which were great alternations of temperature, bad water, and coarse and insufficiently cooked food."

Fortunately the expedition was over before the rainy season set in. Had the troops been detained much longer in the country, in the plains, or before Magdala, the amount of serious sickness would unquestionably have greatly increased. There was a slight amount of scurvy among the Indian troops, but none among the European.

The surgical history of the campaign is soon told. In the actions of the 10th and 13th April, 2 officers and 13 European soldiers were wounded. All did well. Besides these, 17 sepoy soldiers were wounded. Most of the cases were slight; 2 were gunshot wounds in the abdomen, and both proved fatal.

In conclusion, it need only be added that, of the European force, only 333 invalids were sent home to England, either by the overland route in March, or round the Cape in one of the hospital ships, at the close of the campaign.

The account of the severe epidemic of fever in Cape Colony, in 1867, by Staff Surgeon-Major Thornton, is a valuable contribution to public hygiene. Cape Town, where the disease

chiefly prevailed, and to which the description mainly refers, has a population of 30,000 souls, about half European and half coloured; consisting of Malays, Negroes, Hottentots, Kaffirs, Mozambiques, half-castes. For two or three years there had been an unusual amount of destitution and misery among the poor, in consequence of failure of the crops from drought. Filth, personal and domestic, at all times; and now the accumulations round the squalid and overcrowded hovels of the people became greater than ever from the want of the ordinary heavy rains at certain seasons. The supply, too, of water for use, always insufficient, was worse in every respect. In this state of things fever began to make its appearance, as had been anticipated, in June or July; it gradually increased during August, reached its acme in September; after which it began to decline, but did not cease till the following January.

Dr. Thornton estimates, from the imperfect data at his command, that between five and six thousand cases occurred in the town, and that 369 were fatal, or in the ratio of 6·37 per cent. The mortality was much greater among the cases taken into the civil hospital, where it was fully 14 per cent. of the admissions:

“The wretched state in which hundreds of these coloured people were brought to the hospital was fearful. Emaciated to the last degree, crawling with vermin, and with scarcely a rag to cover their nakedness. Several died shortly after admission, and many admitted in a state of delirium.”

Comparatively few of the well-conditioned classes were attacked; and in the garrison, which, including women and children, numbered 1929, only 2 died out of 152 attacked. The nature of the fever appears to have been, in a large number of cases, distinctly typhoid or enteric; although, unfortunately, the diagnosis was not verified, in civil practice, by necroscopic examination. In other instances it was synochus or a mild remittent; and, in a few, it seems to have been petechial typhus. That it was under certain conditions contagious is shown by the fact that—

“All the male and female nurses in both hospitals (civil) caught the infection, and suffered most severely.”

Two, if not more, of the medical men died.

“The curative means used,” says Dr. Laing, physician to one of these hospitals, “were very simple. It was wonderful to witness the rapid improvement also immediately after a warm bath and refreshing sleep produced by morphine and camphor. This was

followed up by quinine, wine, and good soups; and, in most of the cases, the convalescence was complete in about fourteen days."

He adds:

"From the symptoms there were convincing proofs that both cerebral and spinal meningitis generally existed. The intense headache, pains in the cervical and dorsal regions, with, at times, severe tetanic twitches, were almost always present. Vomiting was a troublesome symptom, and paralysis of the bladder often necessitated the use of the catheter. In the cases attended with furious delirium the cold douche was of great service."

The fever existed, at the same time, in other parts of the colony. In Simon's Town, between twenty and thirty miles from Cape Town, the epidemic began in August. Among the civilians were 288 cases and 20 deaths. Among the kroomen in the dockyard, 20 were attacked, and 3 died; and of 16 sailors and soldiers received into the naval hospital, 3 died. The fever in the last-named patients was distinctly of the typhoid or enteric type.

From Port Elizabeth, in Algoa Bay, many hundred miles to the east, Dr. Ronbridge wrote:

"We have had since June cases of irregular remittent, running on the one hand into continued, and on the other into intermittent, fever; without rose spots or mulberry rash, with little or no iliac gurgling, with hæmorrhage from the nose, and occasionally from the vagina; spongy gums, yellow conjunctivæ, high-coloured urine, enlargement of liver and spleen. The remedy quinine. . . The fever is doubtless, malarious, modified by climate and local circumstances, and shading into ague on the one side, while other cases assume every character (including the virulence of yellow fever, with its black vomit, its jaundiced skin, and suppressed urine."

This account of the epidemic fever at Port Elizabeth, differing in type, unquestionably, from that in Cape Town, although synchronous with it, is of great interest when taken in connection with the fact that the fever in Mauritius, which appeared in 1866, acquired greater prevalence and fatality in 1867. It was imagined by some persons that the fever had been conveyed from the one to the other colony. For this conjecture there was not an atom of foundation; nevertheless, the home government actually telegraphed at the time to impose quarantine upon arrivals from Mauritius into the ports of India, and this, too, when no embargo was being laid upon arrivals from Bussorah, although the true plague had broken out in the valley of the Euphrates, under circumstances very similar to those which preceded the outbreak of fever in Cape Town!

With simply directing attention to a very instructive paper,

by Staff-Surgeon Dr. Gore, on the "Medical History of Sierra Leone," since its first occupation as a colonial settlement, we must now close our notice of this volume of the 'Army Medical Reports.'

X.—Tuberculosis and Pulmonary Consumption.¹

It is now just twenty years since Lebert published his work on scrofula and tubercle, which was thought to establish the doctrines of Bayle and Læennec upon a firm basis of microscopical observation; and yet we may date from it the beginning of a complete revolution, which has long since caused Lebert himself to abandon his opinions—so rapid, in these railroad days, is the tortuous course of medicine!

There were, at that time, great and important differences in the theories entertained as to the nature of tubercle; but, had any one been bold enough to raise the question, there would have been almost complete unanimity in holding that phthisis first began to be accurately known at the end of the last century. When the physicians of this generation heard their elders express their opinions of the causes and varieties of pulmonary consumption, they received these, it is to be feared, with polite incredulity at the best, or, perhaps, with a languid curiosity, as the worthless traditions of a bygone day, before the stethoscope and the microscope had reformed our theory and our practice. Thus, we well remember how, on first reading, perhaps, the most suggestive of modern medical works, we were startled by what seemed the paradoxical absurdity of the following sentence, endorsed by one of the greatest names in practical medicine.

"If we are certain of any one thing, it is this, that pulmonary phthisis, though less well known formerly than now as to its charac-

1. ¹ FELIX VON NIEMEYER's *Klinische Vorträge über die Lungenschwindsucht*: mitgetheilt von Dr. OTT. Zweite auflage. Berlin, 1867. Pp. 112.

Clinical Lectures on Pulmonary Consumption. By FELIX VON NIEMEYER, Professor in the University of Tübingen: published by Dr. OTT. 2nd Edition.

2. *Die Tuberculose, die Lungenschwindsucht und Scrofulose; nach historischen und experimentellen studien bearbeitet.* Von Dr. L. WALDENBURG, Privatdocent an der K. Univ. zu Berlin. Berlin, 1869.

Tuberculosis, Pulmonary Consumption, and Scrofula, studied by historical and experimental investigation. By Dr. L. WALDENBURG, Tutor in the University of Berlin.

ters and its anatomical varieties and phases, was, on the other hand, much better known than as to its etiology, its pathological varieties and phases, its therapeutical indications, and its treatment.”¹

Probably all who read this sentence when it was first written shared our feeling with regard to it; *now* it would certainly be accepted by every one who accepts the most recent doctrines on this subject. These have many disciples in England, where Dr. Andrew Clark and others have laboured to spread sounder views of the nature of phthisis; but their most decided exponents are to be found in Germany, and their writings are not always accessible to Englishmen.² Hence we believe that an account of the two works we have placed at the head of this article will be read with interest, since they may be taken as representatives of the most advanced school, and together cover all the ground recently gained.

In order to understand their position, it will be necessary to go back a few years, and to recall to mind the state of science at the time Lebert published his work.

The Paris School of Medicine still retained that pre-eminence in pathology which it had enjoyed for half a century; and its teachers were divided into two schools of opinion as to the nature of tubercle. The more numerous party held it to be a specific morbid product, arising from some peculiar diathesis, or constitutional fault; and that pulmonary consumption was caused solely by the deposition of this substance. The earliest trace of this view seems to be found in the writings of Morton, in the seventeenth century; but it was first clearly stated by Bayle, who was followed by Lænnec with even more confidence. Their joint authority caused it to be generally received, and, when supported by the careful observations of Louis, and Lebert's supposed discovery of the tubercle-corpuscle, it was looked upon as fully established.

A minority of illustrious teachers maintained from the first a different opinion. The perfect symmetry of Broussais' system left no room for the admission of tubercle, and he was accordingly led to seek another explanation, which he defended with his wonted ingenuity and decision. He taught that tubercle was merely a caseiform variety of pus, produced by inflammation of the capillaries in the lymphatic glands; that phthisis was a chronic pneumonia, “une inflammation lymphatique du poumon.” This part of his teaching would have shared the

¹ Trousseau et Pidoux, *Traité de Thérapeutique*, art. Hysope.

² Niemeyer's view will be found, in part, in his recently translated work on the ‘Practice of Medicine.’ His pamphlet has been translated into French, and was very ably reviewed by M. Lasègue, in the *Archives de Médecine*, for June, 1867.

sudden downfall of the whole system had not Andral been able to support it by actually observed facts. He was able to trace caseous masses in the lung to their origin, which he found was usually pus; and, as he believed caseous matter to be tubercle "par excellence," he supposed Bayle's grey granulations were simply circumscribed inflammations of the air-vesicles, terminating in formation of "tuberculous matter," or "scrofulous pus," in consequence of some diathetic predisposition. Cruveilhier maintains an almost identical opinion in his last great work.

These two opposed views as to the nature of tubercle prevailed at the middle of the present century, in this country as well as France and Germany, Lænnec's opinions being held by a large and confident majority, while Andral was followed, more or less closely, by Schröder van der Kolk, Stokes, and Blakiston. Dr. Addison, perhaps, did more than any one else in England to advance the doctrine of "pneumonic phthisis," but his opinions received less attention than they deserved, owing to the comparative simplicity of the "constitutional" theory. Such was the state of science when Lebert published his 'Microscopical Researches,' which at the time appeared so decisive. Their real importance was of quite another kind, and consisted in this, that they called attention to a verbal fallacy which was common to both schools, and was at the bottom of their divergence. This dated from Portal and Bayle, who had called all cheesy accumulations in the lung "tubercles," which use of the word, harmless so long as it was employed only to denote masses of a generally similar appearance, became very mischievous when Bayle and Lænnec went on to describe grey granulations as "tuberculous." It was injurious, because it prejudged the question whether all cheesy masses thus originated, and, by supposing that a similar appearance implied a similar beginning and progress, decided a very important question, without even any clear idea of its existence. Matthew Baillie's use of the adjective "scrofulous," in the same sense, was certainly preferable, but by no means unobjectionable, as it also begged an undetermined question, by implying that the formation of cheesy matter was due to the diathesis called scrofula.

Craigie's proposal of a new word ("tyrosis," from τυρός, cheese) to signify the caseous nature of certain accumulations, without any reference to their origin or causes, was a sign of better things, but hardly attracted notice at the time.

Meanwhile, Virchow was beginning to teach that caseous matter was not specific in its nature, but was simply thickened

pus, the result of ordinary inflammation.¹ Had this been all, he would have done no more to advance the state of science than Reinhardt, who about that time (1847-50) held the same opinions. But the publication of Lebert's book led him to reconsider the whole question of tuberculosis, and to discover that each of the prevailing opinions was based on a certain amount of truth. On the one hand, it was clear that caseous matter was sometimes produced by inflammation; on the other hand, it was equally plain that the semi-transparent grey granulations were to be looked upon as primary, new products, and afterwards underwent the caseous change. He was enabled to reconcile these two series of facts by the further observation that the change which he still called tuberculisation was produced not merely in these two cases, but in many others, and was to be looked upon as a form of degeneration, like the fatty, waxy, atheromatous, or cretaceous change; a partial death, "or rather, a necrobiosis." In 1852, he first suggested that the word "tubercle" had better be confined to those new products which begin as semi-transparent, grey granulations, while the change which may occur in them, in ordinary exudation-matter, in typhous matter, in carcinoma, and in other pathological products, should be denoted by the more general term "caseation" ("verkäsung"), and the result termed "caseous" or cheesy matter.

The best proof that some change in nomenclature was needed, is afforded by the course of things in France. Although some French microscopists (*e.g.* Mandl) held from the first that Lebert's tubercle-corpuscles were not specific, they were generally looked upon as such, and caseiform matter was still considered to be tubercle, *κατ'ἐξοχὴν*. Hence, as soon as it was found that what had hitherto been called "miliary tubercle" neither contained tubercle-corpuscles when examined with the microscope, nor appeared caseous to the naked eye, Bouchut, Robin, and others, were perfectly logical in asserting that these grey granulations were not tuberculous.

So far they were dealing simply with facts, but this mode of interpreting them has led Empis to set apart the old "miliary tubercles" or grey granulations as a new disease, under the name of "granulie," or "maladie granuleuse," which he believes to originate in some specific form of inflammation. It might be supposed from this that Virchow and Empis were in substantial agreement, and that the differences between them were simply verbal; but there is this real novelty in the latter's pathology—

¹ It is remarkable how in this, as in so many other points, Virchow is the successor of Broussais, whose theories he develops and corrects.

that he considers miliary tubercle to be in no way connected with yellow caseous matter, and that whenever this change takes place, a fresh morbid product must have been deposited; in Empis' language, "granulie" must have been complicated by tuberculosis, and this complication is the only dangerous possibility in the disease.

Virchow's solution of the differences which had so long prevailed was generally accepted in Germany, and even in France, by Vulpian, and Hérard, and Cornil. We shall mention, by and by, the principal theories of the nature of tubercle which then arose, but the practical results were at once considerable. Being set free from the postulate common to the two schools, that the presence of tubercle was the anatomical character of phthisis, practical men began to examine the facts, with a view to ascertain their new significance.

One of the first to do so was Buhl, who published, in 1857, his 'Report of 280 Post-mortems.'¹ He divided his cases into—1. Acute miliary tuberculosis. 2. Acute infiltration (lobular pneumonia). 3. Chronic tuberculosis (wrongly named, seeing that he considered it to be dependent on a pneumonia or a bronchitis.) He carefully examined 23 cases of acute tuberculosis, and found in all but one pre-existing masses of caseous matter or pulmonary cavities. Rokitsky had already observed some such connection, but without attributing the same importance to it as Buhl, who taught that true tuberculosis was a specific disease caused by a special poison in the blood from absorption of caseous matter, running the course of such diseases, and producing miliary tubercle as its characteristic eruption.

C. Hofmann has more recently² gone over the same ground, and proved that miliary tuberculosis almost invariably occurs where caseous masses pre-exist; these being found, not merely in the lung, but in the urinary and genital organs, in the lymphatic glands, and among the products of suppurative peritonitis. Hofmann differs from Buhl, in not looking upon tuberculosis as a specific disease; the very diverse origin of the masses of detritus, which are the starting-point of the disease, would seem to him to suggest that some common form or quality of the caseous masses, and not anything specific in them, is the true cause.

We have probably already wearied our readers before coming to Niemeyer's pamphlet, but it seemed essential to clearly define the "stand-point" of the Tübingen professor. Niemeyer is, in

¹ 'Zeitschr. für rat. Med.,' 1857.

² 'Archiv. für Klin. Med.,' 1867. It is only fair to add, that Klebs and Valentin deny the frequency of this connection between the two morbid states.

fact, a clinical observer, bringing to the test of actual practice the pathological theories of Virchow and Buhl. The opening sentence of his lectures will prove this, and will also testify to his small respect for existing opinions.

"No doctrine," he says, "in the whole of pathology so urgently needs a reform as that of pulmonary phthisis; in this point clinical medicine has been far outstripped by pathological anatomy." Niemeyer accepts Buhl's facts as to the connection between tuberculosis and caseous masses, but thinks it overstrained to say that this connection is due to a blood-poisoning, and is constant. He rather supposes that, in chronic tuberculosis at least, the action of the cheesy accumulations is local and produced by means of the lymphatics.

Let us begin by examining the probable sources of tyrosis, to use Craigie's word. Our author opposes very decidedly the doctrine that caseous infiltration of the lung is only due to one special kind of pneumonia. "On the contrary, it may be asserted with perfect truth that *every* form of pneumonia, under certain circumstances, terminates in caseous infiltration, and that this is not the constant and sole termination of *any* form of disease." This has at any rate the advantage of simplicity, and unless it be disproved, it had better be held provisionally than the contrary doctrine of scrofulous pneumonia, which is more likely to serve as a cloak for imperfect and careless observation.

Ordinary acute pneumonia ("croupous" pneumonia, as the Germans absurdly call it) rarely terminates in tyrosis, except in persons who have long been suffering from emphysema.

This result is much more common in the case of acute catarrhal pneumonia, as described by Meschede, Ziemssen, and Bastels. In this disease the alveoli are blocked up by numerous young cells, which in favorable cases undergo fatty degeneration and disintegrate rapidly, so that the alveoli become once more permeable. But not unfrequently the cells continue to collect, and become caseous from loss of water and imperfect fat-change. This form of lung-inflammation may spread from the alveoli to the pulmonary parenchyma, and break it up rapidly, with all the symptoms of "phthisis florida." It is especially common as a result of measles or whooping-cough, and the numerous deaths in such cases generally ascribed to tuberculosis, are for the most part due to intercurrent catarrhal inflammation of the lung.

Chronic catarrhal pneumonia is, Niemeyer tells us, a very common disease. It includes the so-called "infiltrated tubercle," "tuberculous infiltration," "tuberculous pneumonia," &c.; and

a correct nomenclature is here important, as a wrong idea of the nature of the disease affects prophylaxis and treatment.

The cells effused in this variety of pneumonia tend, as a rule, to undergo the caseous change; but the cavities are not invariably produced; more often the cells are absorbed, or cretaceous masses are formed. In itself, then, chronic catarrhal pneumonia is not a very fatal disease, and "the greatest danger for most phthisical patients is, that they may become tuberculous," tuberculosis being, for our author as for Buhl, a secondary affection, produced by caseous morbid products.

Such being Niemeyer's pathological principles, let us next see their practical application. And, first, as to the causes: there is no certain evidence that *tuberculosis* is hereditary. For this it would be necessary to show that one parent was *really* tuberculous (in Virchow's sense of the word) at the time of conception, and that the child became so, without having any other disease capable of producing it. Great stress has been laid by Virchow and others upon the hereditary character of tuberculosis of the meninges; but even here it can generally be shown that it is secondary to caseous formation elsewhere, especially in the bronchial glands.

On the other hand, an hereditary predisposition to *pulmonary phthisis* is not only certain, but frequent; and this also is worked out, not directly, but by weakness and vulnerability of the constitution.

As to the influence of vaccination, Niemeyer would probably adopt the conclusion of Rilliet and Barthez—"Que la vaccine favorise, très probablement, la prédisposition aux tubercules," for he teaches that it may act like all other diseases in young children. Hence, although he strongly condemns the "objectionable and dangerous agitation" against vaccination in general, yet it should not be indiscriminately permitted in weak children. Probably few practical questions will be more affected by the views we have been expounding than this of vaccination; careful discrimination between true tuberculosis and caseous formations being necessary.

The most important cause predisposing to these lung alterations is affirmed by our author to be extreme vulnerability of constitution, as evidenced by repeated illnesses in early life. Scrofula acts, partly by this increased vulnerability, partly by the formation of caseous masses, which may lead to *tubercular phthisis*.

The general opinion among physicians, that phthisis is independent of what are called exciting causes, and is due only to a diathesis, Niemeyer holds to be as unfounded as it is dangerous; and he remarks that the public are fortunately more careful in

this respect than most of their medical advisers would theoretically require.

Among the exciting causes of pneumonic processes which produce phthisis, our author lays particular stress on the effect of bodily exertion in disturbing the circulation, telling us that he has, among his notes of cases, "a series of instances, in which the first symptoms of phthisis followed immediately on immoderate dancing, or other similar exertion, without any evidence of chill at the same time." This is a point well worth considering, but far more important is the influence of hæmoptysis. It was held, from Hippocrates down to the beginning of the present century, that blood remaining in the bronchia after hæmoptysis was a frequent cause of phthisis.

Laënnec first offered the explanation of the facts which is now generally received, viz., that both the hæmoptysis and the lung affection are effects of the deposition of tubercle. This was merely a logical corollary of his general teaching; but it is remarkable that when men abandoned this they did not reconsider the older view. Niemeyer has done so, and his opinion is so well worth weighing, that we give it in his own words, at the risk of being thought too lavish in our quotations:

"I have no hesitation in saying that, in most cases, hæmoptysis is followed by a more or less decided irritation of the lung and pleura. Since my attention has been called to the occurrence of these consecutive pleuropneumonix, I have almost without exception been able to find, on the second or third day after the hæmorrhage, an increased frequency of the pulse, a heightened temperature, general *malaise*, more or less severe lancinating pain in the lateral regions of the chest, and frequently fine bubbling rhonchi, friction-sounds, or a slight percussion-dulness, with weakened vesicular murmur, or with bronchial breathing" (p. 51).

Inflammation of the lung thus produced ends ordinarily by resolution; but it may go on to tyrosis, and perhaps thence to the deposition of tubercle. May we not suggest that tuberculosis is, perhaps, producible directly by absorption of the decomposed blood?

Of course Niemeyer does not intend to say that hæmoptysis generally occurs in a hitherto healthy lung: it most frequently happens in the course of phthisis, and is an evidence, when produced by very slight causes, of an abnormal condition of the bronchial circulation, even if no other disease can be detected. Traube supports Laënnec's teaching, in opposition to Niemeyer's; but we believe that many practical men, on reviewing their notes or impressions of cases, will be induced to adopt the latter,

and to reinstate Morton's "phthisis ab hæmoptœe."¹ Dr. Waldenburg has performed some experiments to test the question, by injecting fresh warm blood into the trachea in rabbits. When the blood was derived from another animal, the irritation produced was so violent, that death speedily ensued: but when blood previously taken from the same animal was employed, it lived five days, and after death there were found, caseous inflammation of the lung and liver, pericarditis, peritonitis, and suppurative pleurisy.

If we accept this mode of origin as possible, our practice will be seriously modified: without adopting the energetic treatment which Galen employed in all such cases, we shall be prepared to look for signs of intra-thoracic inflammation, and to meet it as it arises; and in this way it is to be hoped that some of the most fatal cases of consumption may be checked in the beginning.

If we proceed now to review Niemeyer's sketch of the symptoms of phthisis, we shall find some points of equal importance. The features of the disease will vary according to whether the case be one of inflammation of the lung alone, or of pneumonia complicated with tuberculosis, or of tuberculosis only. "These three forms can, in most cases, be distinguished with approximative accuracy," as will be seen by the following sketch.

I. *Symptoms produced by pneumonic processes.*—Lung inflammation of a destructive character may be either acute or chronic. The former is produced, sometimes by hæmoptysis, sometimes by acute pneumonia, and sometimes by an extensive bronchial catarrh. We have just quoted Niemeyer's description of the symptoms which he has observed after hæmoptysis, and which betoken pleuropneumonia: in such cases more or less of the lung-substance is usually destroyed.

A similar result may be feared in acute pneumonia, when the fever does not cease at the end of the first, or beginning of the second, week; when it only remits in the morning, with considerable sweating; when percussion-dulness persists, and liquid clanging rhonchi are still heard for a time over the affected part. The sputa are mucopurulent; and when they contain elastic fibres, it will be evident that the diseased lung is breaking up. Most of these patients die in a few weeks; but sometimes, the fever ceases, the expectoration diminishes, and the patient improves, retaining signs of induration and retraction of the affected part, and often evidences of a cavity.

¹ See a paper by Dr. H. Weber, in vol. ii, of the Clinical Society's Transactions.

It will become difficult to tell when an acute catarrh is likely to terminate in destruction of the lung; but the comparative gravity and endurance of the symptoms may afford some ground for conjecture, which will be strengthened if the sputa become rusty, and there be physical signs of pleurisy or consolidation of the lung. Most cases of "galloping" phthisis belong to this category, and have, according to Niemeyer, begun with bronchial or alveolar catarrh. When the affection has been limited, recovery, with absorption of the caseous products, and consequent signs of contraction, is not uncommon. The sinking-in of the supra- and infra-clavicular fossæ, and the low level of the apex of the lung, so often observed in consumptive patients, is generally due to an attack of this kind, and people may be seen who have had several such illnesses, the percussion-dulness, and retraction of the chest-wall, being increased by each.

Or again, destructive pneumonia may be chronic in its course from the very first, and may begin very insidiously. A slight catarrh is usually the first symptom; and, if the patient's general health should seem to suffer after a time, if he should become thin and lose his appetite, the chest should be frequently examined for signs of consolidation or softening of the lung, and careful thermometrical observations should be made, with a view to ascertain the presence or absence of fever, which is a constant accompaniment of the disease if at all extensive. This chronic form of catarrhal pneumonia tends to recovery, if the external circumstances are favorable; or to extension, if they are the reverse; and the most typical examples of it are those persons who are admitted into a hospital one winter after another, improving under care and medical treatment, but each fresh attack spreading the disease over a greater extent of lung.

II. *Pneumonic phthisis complicated with tuberculosis*.—Our author teaches that the occurrence of tuberculosis is the great danger for all consumptive persons, and one which should make us reluctant to give an absolutely favorable prognosis in any such case. The occurrence of this formidable complication may be perfectly latent, and will usually be very difficult to detect; it may be suspected, if, the physical signs remaining much the same, the dyspnœa and fever become increased. Niemeyer thinks that the fever also changes in type, becoming continuous instead of remittent. The only evidence of tuberculosis which is less equivocal is the affection of other organs, generally the brain in children, and the larynx or intestines in adults.

III. *Primary tubercular phthisis*.—Here the first symptom is generally considerable increase of temperature and waste of the body; cough and expectoration being only observed later.

If the breath is at the same time very short, and the physical signs almost negative, the diagnosis will be almost certain. A little later there will be local evidences of consolidation and destruction of the lung-substance, though these are not usually so considerable as in pneumonic consumption. The voice generally becomes hoarse at an early period of the disease, and tubercles are deposited in the intestines. The tendency of this form of consumption is towards death, which generally occurs in a few months, no medical treatment being of any avail.

It will be seen, therefore, that the important point for discriminating tubercular from pneumonic phthisis seems to be the considerable amount of constitutional disturbance relatively to the evidences of local mischief. The most difficult problem will be when a patient with signs of consolidation or excavation, and also with considerable general symptoms, comes before us, to determine the nature of the case; but if any accurate history exists, the order of events will be of use to us, the general symptoms having preceded the local signs in tubercular phthisis, and having accompanied or followed them in the pneumonic variety.

These views as to the symptoms and nature of pulmonary consumption have a very important bearing on treatment; but we think that will be more usefully discussed after we have noticed the other work at the head of this article.

It will have been observed that we have almost disregarded two most important pathological questions, viz., what are the causes of the caseous change in phthisis, and what is the nature and cause of tubercle?

Dr. Waldenburg, the able editor of the *Berliner klinische Wochenschrift*, supplies us with a great amount of material towards answering both of these. The first part of his work is occupied by an historical account of the theories which have been put forth, from the times of Hippocrates to our own day, in order to explain the phenomena of phthisis, tuberculosis, and scrofula. We have borrowed very largely from this excellent résumé, which is thoroughly German in its elaboration and completeness, but very un-German in its clear and pleasant style. The only fault we have to find with it is, that the author treats English writers far more inadequately than his own countrymen or Frenchmen; in fact, we imagine that he is not acquainted with our language, and only knows our medical works by means of quotations in Virchow's writings, or by translations.

In the latter half of the book he develops his own opinions as to the pathology of phthisis and tuberculosis; and his views are sufficiently interesting to merit a detailed examination.

And first, as to the cause of the caseous degeneration which

the morbid products of phthisis—whether pneumonic or tuberculous—undergo.

There appears to be no doubt that this is due to some constitutional predisposition, the strongest evidence being that, in some of the animal races, all ordinary pus undergoes this change. Our author believes with Engel, that tyrosis is essentially produced by abstraction of the watery portions of pus, and he has collected several probable arguments for this view. Thus, he remarks that rabbits, who are particularly liable to this form of degeneration, never drink water, and take little or no liquid food, their urine is highly saturated, and their excrement very hard and dry.

But since the tendency to tyrosis is in human beings particularly manifested in the lungs, it must be brought about by some local anomaly of structure or function. Dr. Waldenburg suggests, as a mere hypothesis, that this is, perhaps, a relatively small amount of blood in the pulmonary vessels, and a consequent “dryness” of the pulmonary tissue. This would account for the immunity from phthisis conferred by certain diseases of the heart and great vessels. Rokitansky long ago remarked that the venous and cyanotic blood-crises were opposed to tuberculosis (*i.e.* phthisis); and Traube has more recently interpreted his facts to mean that only those diseases of the heart and great vessels protect from phthisis, which hinder the outflow of the blood from the pulmonary veins. On the other hand, he has repeatedly observed phthisis in other heart diseases, and has seen it particularly concur with stenosis of the pulmonary artery. Some of our readers will remember that Lebert has quite recently¹ come to the same conclusion, which had also been noted by the older English and French observers.

The rarity of phthisis in mountainous countries, and in persons whose chests have been thoroughly expanded, point in the same direction. So do the immunity enjoyed by those who suffer from chronic bronchitis, and the frequency with which phthisis follows pleuritic compression.

The next question to consider is, what is the origin of tubercle, in the strict sense of that word? The principal means that have been taken to answer the question are direct experiment by inoculation. Before detailing the results of his own experiments, Dr. Waldenburg gives us a short account of those hitherto performed. We do not propose to follow him in this; but we may note, as a curiosity, that Lænnec accidentally pricked his hand with a splinter from a tuberculous vertebra, and, after some local inflammation, there was formed “a small,

¹ ‘Med Times and Gaz.,’ Dec. 11, 1869.

yellowish, hard body, which closely resembled a crude tubercle." Is this an instance of tubercular formation at the point inoculated, such as Wilson Fox and Burdon Sanderson have particularly noticed in their experiments?

Dr. Waldenburg has performed in all 104 experimental inoculations, on 71 rabbits, 28 guinea-pigs, 1 hedgehog, 1 goat, and 3 horses. We propose only to dwell upon the general inferences deducible from these experiments. They were for the most part performed by making a small incision through the skin, and inserting a small portion of the substance to be inoculated in the subcutaneous cellular tissue. The preparations seem to have been repeatedly submitted to the Berlin Medical Society, and those who are still incredulous as to the identity of the miliary formations thus produced with real tubercle, may be interested in learning that they were examined by Virchow, who has stated that he is convinced of it. Our author further remarks that he has taken every possible means to avoid sources of error, these being, principally, collections of entozoa (especially Psorosperms or Pentastomes) which can be distinguished by microscopical examination, and spontaneous tubercle, which is very rare in the rabbit and guinea-pig, and is then confined to one organ, generally the liver.

1. In one series of experiments 18 rabbits and 2 guinea-pigs were inoculated with true miliary tubercle; ten of these died of septicæmia soon after the operation, and in eight of the remaining cases an eruption of miliary tubercle was produced. The tuberculous rabbits lived from ten days to two months; the former being the earliest period at which the disease shows itself. There appeared to be no connection between the local state of the wound and the infection of the system; in most of the animals which died of septicæmia the wound suppurred, while in the most extensively tuberculized animal it had perfectly healed, and could not be detected. In the great majority of the successful cases the tuberculous matter was already softened or caseous; on the other hand, almost all those inoculated with completely unsoftened tubercle gave negative results.

2. In another series of experiments six rabbits were inoculated with perfectly fresh cheesy matter from non-tuberculous lymphatic glands, received from a scrofulous patient when alive. One of these died of pneumonia—not an uncommon thing in winter; the remaining five presented more or less extensive tubercular deposit, more than one organ being affected in each of them. These miliary tubercles were in all respects similar to those produced in the first series of experiments.

3. In another series various non-tubercular morbid products were employed. Thus carcinoma was inoculated eight times,

with unimportant results, except that when schirrus from the living body was employed, suppuration seemed to be replaced by local swelling with formation of large cells rapidly undergoing fatty degeneration. In one of these rabbits, a hard nodulated tumour of the size of a small fist formed in about six weeks, with a nodule the size of a walnut below it; but, unfortunately, the animal was then stolen, and its further progress was not observed.

Common hepatized lung and catarrhal sputa produced death in a few days from septicaemia; hence the results may be looked upon as purely negative.

Finally, three rabbits were inoculated with pus from an abscess intentionally produced in another; two died in a few days with no positive results, but the third, who was repeatedly inoculated, died, after some time, tuberculous. Of three guinea-pigs similarly treated, two lived for some months, and were repeatedly inoculated; after death they were found to present signs of several successive tuberculous eruptions.

These last cases clearly show that ordinary pus, of traumatic origin, can produce tuberculosis when inoculated; but it is to be remarked that it does so much less frequently than when miliary tubercle or caseous matter is used. In these cases the local state was in striking contrast to the general affection. In those animals which were most extensively tuberculized there was no sign of formation of pus near the wound; while those which had considerable local suppuration were free from all affection of the internal organs.

4. In order to discover whether tuberculous and caseous matter owed their power to the presence of some chemical virus, six rabbits were inoculated with miliary tubercle, or non-tuberculous caseous matter which had been preserved for some months in spirits, and finally washed in water. One of these died sixteen days after the inoculation, with no evidence of tuberculosis; but in the other five more or less extensive miliary deposit was noticed after death, so that the results thus obtained are more constant than when fresh matter is employed, evidently on account of the absence of septicaemia. Non-tuberculous sputum, preserved for some time in spirit, produced general tuberculosis three times out of four.

5. It may be said that the poison, whatever its nature, is sufficiently stable to be unaltered by long keeping in spirit. This is improbable; but, in order to test its truth, tuberculous and caseous matter was inoculated after being boiled or having been left in contact with nitric acid, permanganate of potash, or chromic acid. Six experiments were performed, and in half the number tuberculosis was produced. These are too few to serve as a basis for

a positive opinion, but Dr. Waldenburg was led to conclude that the infecting power of the morbid product so prepared depended rather on its retaining its morphological than its chemical elements. Further experiments in this direction are certainly desirable, as these are far from conclusive.

6. In order to ascertain whether the solid elements inoculated are actually conveyed to the seat of the miliary formation, Dr. Waldenburg inoculated eight times miliary tubercle, or non-tuberculous, caseous matter, mixed with aniline-blue. He detected this colouring matter in by far the larger number of the miliary collections thus produced which he examined, and also found it in the most various parts of the body; generally, in connection with the white blood-corpuscles, in the cellular tissue of the lung, in the hepatic cells, or in the renal epithelium. The inoculation of aniline-blue or carmine alone produced in one case miliary accumulations, in all respects resembling tubercle; but the ordinary result seemed to be numerous foci of inflammation in the lungs or other internal organs. Dr. Waldenburg is at present engaged in studying the early history of the development of tubercle, by means of inoculation of coloured morbid products, and he recommends other investigators to do the same. He gives us one instance, of a young goat whom he inoculated seven times with tubercular caseous matter, coloured with carmine. Numerous miliary formations here took place in the subcutaneous cellular tissue at the places of inoculation; the lymphatic glands were also affected, as well as the internal organs; in most of the tubercles thus produced the carmine-colouring could be plainly distinguished.

We have dwelt so long on the actual experiments that we can give but little space to the conclusions to which they seem to point. The author himself considers his experiments sufficiently disprove Villemin's opinion that tubercle is a specific morbid poison in the same sense as syphilis is; but he does not, on the other hand, adopt Lebert's latest doctrine of the absolute non-specific nature of that product. So far, he is in accordance with the majority of authors; but he differs from them in his further interpretation of facts.

The doctrine of Rokitansky, that tubercle is the product of a special blood-crisis, with such modifications as the progress of science requires, has been accepted by most German physicians. It is held nearly in its original form by Dittrich and his disciples, who have been most active in spreading it, by their researches into the connection of other diseases with phthisis; Buhl's teaching, which we have referred to above, is a more modern variety of the same fundamental doctrine.

Cohnheim, of Berlin (whose name is so well known now by

his theory of suppuration), has been experimenting recently with Dr. Fränkel, and comes to the conclusion that caseous pus, produced at the place of inoculation, is the sole cause of tubercular deposition. Dr. Wilson Fox inclines to the same opinion, which yet appears to require more evidence before it can be received. Dr. Waldenburg's success in producing tuberculosis by the inoculation of healthy pus seems decidedly opposed to any such conclusion; and his own inference from his experiments seems to us the safest in the present state of our knowledge. In his own words, it is this:

"Miliary tuberculosis is a resorption-disease, consisting in the taking up into the circulation of very minute corpuscular elements, and in their deposition by nodular formation in numerous separate parts of the various organs. Hence tuberculosis is a general disease, in a certain sense also a blood-disease, although not a specific one. It stands in the nosological system nearest to pyæmia."

The difference being that, in pyæmia, the particles taken up into the circulation are larger, and so produce embolism, stasis, abscess, and local gangrene, besides causing violent irritation and toxic symptoms from their putrid condition.

Hofmann, whom we have quoted above, has arrived at an almost identical conclusion from the stand-point of pathological anatomy; and we may add that the researches of one of the best French morbid anatomists might be quoted in support of it. M. Cornil considers that slackening of the local circulation is the primary fact in the formation of miliary tubercles, as is evidenced by the collection of white blood-corpuscles on the vascular walls.¹ Now that Cohnheim has established the possibility of extravasation taking place in the capillaries, it is probable that tubercle is produced in the same way as he has shown pus may be. This hypothesis is further supported by the predilection which tubercle has for the immediate neighbourhood of the smaller arteries.

Our knowledge of the processes of inflammation is so limited that we cannot venture as yet to say whether it and tuberculosis are not identical in their earliest stages, as M. Morel² and others believe.

So much for the mode in which tubercle originates; there would then remain the question as to its nature when developed, which Dr. Burdon-Sanderson has so completely examined. It would carry us far beyond the limits of a review, if we were to examine the theory of hypertrophy of the adenoid tissue at

¹ Cornil et Ranvier, '*Manuel d'Histologie Pathologique*,' p. 204.

² '*Traité d'Histologie Humaine*,' p. 56.

all sufficiently; nor does Dr. Waldenburg's work throw any light on the subjects.¹ There are several conceivable ways in which we can imagine that their respective views might be harmonised; but as they are purely conjectural, we spare their enumeration to our readers.

We would rather direct their attention to the more attractive subject, of the practical bearing of the opinions we have hitherto related.

And first, as to the prophylaxis of phthisis. This resolves itself into a more simple question—how can the tendency to the formation of caseous products be prevented; for, as far as we know at present, these are the ordinary antecedents of tubercle. The hygienic treatment to be recommended here is the same as that usually laid down for scrofula, the two affections being, in part at least, identical. In order to protect the lungs specially from this form of disease, abundance of out-door exercise, fresh bracing air, and expansion of the chest, should be the means employed. Their rationale is perfectly intelligible on Dr. Waldenburg's theory, which we have given above.

When caseous matter has formed in any part of the system, the advisability of its speedy removal should be seriously considered. The purulent inflammations which seem most frequently to produce an attack of tuberculosis are pleurisy and peritonitis, caseous inflammations of the urinary or genital organs, of the joints (especially the hip), and of bone (especially the petrous portion of the temporal).²

Mr. Holmes's excellent remarks on the removal of local disease before the system is affected have been already referred to in a former number of this Review, but they acquire here a further significance. Surgeons can best judge how far they will bear extending to the removal of diseased tonsils, or of caseous lymphatic glands, which seems to be now a frequent operation at Berlin.

Supposing phthisis actually established, our first question would of course be, how far is it curable? Both our authors answer this by pointing out that the doctrines they advocate are far more hopeful than those of Lænnec, and should be an inducement to far more careful treatment. The word phthisis cannot now be interpreted to imply the existence of a hopeless diathetic

¹ He quotes a paper by Knauff of Heidelberg, apparently unknown to Dr. Sanderson, in which the same views seem to be advocated; but we have hitherto been unable to consult it.

² We may here remark, in passing, that in the great majority of these affections the morbid products never come in contact with the air; this seems a *primâ facie* objection to Dr. Burdon-Sanderson's suggestion, that the admission of air into the wound is necessary for the artificial production of tuberculosis.

disease; it is merely a clinically valuable expression for several widely different diseases. Niemeyer has expressed his belief that, if they are treated early and judiciously, most cases of pneumonic phthisis are curable, and there can be little reason to doubt this is substantially correct. Perfect rest in bed will often prevent an acute catarrh spreading from the bronchi to the air-cells, or will check the exacerbations of the chronic form of the disease. Niemeyer attaches great importance to diminishing the fever which wears out the patient's strength, and he finds that digitalis and quinine combined are very effectual for this purpose. We have ourselves been more successful with arsenic and aconite; but we are not able to suggest any precise indications for their employment. When caseous matter has formed, its early evacuation should be encouraged; and this is, perhaps, one advantage of a warmer climate, that the occurrence of tuberculosis is much rarer than with us.

Niemeyer is unacquainted with any remedy which has any effect on true tubercular phthisis; but there is considerable reason to hope that even this is sometimes curable. There are already a certain number of facts which prove that recovery may take place after deposit of true miliary tubercle. Waldenburg has observed several cases among the subjects of his experiments, where complete recovery took place; which coincided with the beginning of spring or summer, and an abundant supply of green food; and these may afford a hint to us of the means which are likely to be of use. At any rate we should try to diminish the fever, and to relieve symptoms as they arise.

This is, indeed, one of the chief morals of this important question, that the worst thing we can do is to fold our arms, and to confine ourselves to a "*vana mortis meditatio*."

The other moral is scarcely less valuable; it is that the importance of choosing correct scientific names can hardly be overestimated, but that we must always strive to look beyond them to the facts they are intended to represent. The injurious effect of Bayle's double use of the word "*tuberculous*," and the advance effected by Virchow's improved nomenclature, are striking examples of Hobbes's aphorism—"Words are the counters of wise men, and the money of fools."

J. R. GASQUET.

XI.—The Growth of Lunacy.¹

REPORTS are the order of the day. Each public department, public institution, and philanthropic society issues reports which are supposed to convey to their readers a more or less complete account of their operations for the past, and of their schemes for the future. The mass of such current literature is enormous, but a greater part of it expires in its birth, either from its own inherent weakness, or from failing to secure to itself an appreciative public. Lunacy reports are launched into the world under favorable auspices, if Government sanction and the official blue covers are to be considered as such; for there is a class, and not a small one, of unenlightened Englishmen to whom a blue cover is an apology for indifference and neglect of the matter enshrined within it. Very reprehensible would it be to express sympathy for such uninstructed persons; yet, withal, the student of government literature will at times deplore the infliction of octavos and folios upon his attention, redundant in words, but sadly wanting in facts and opinions deserving notice.

The official reports of the Lunacy Commissioners of the three divisions of the United Kingdom annually issued, form no mean sized volumes, which, though enveloped in the distinctive blue covers, really demand the attention of the most inveterate anti-cerulean readers who can feel an interest in lunacy matters. And assuredly, in the present aspect of lunacy in our country, no individual can be indifferent to the future of the insane, or fail to experience an interest in the important State question, what is to be done with our lunatics? There is no denying or explaining away the fact of the rapid accumulation of insane persons in all parts of the kingdom, demanding, or supposed to demand, treatment in special asylums; and as yet the vast expenditure incurred in providing asylum accommodation for every county and borough, and which formerly was fondly supposed would suffice for many a long year, has signally failed to secure its object, insomuch so that at the present day the proportion of lunatics unfurnished with such accommodation, approaches, if it do not equal, that existing when the Act of 1843 first called upon counties and boroughs to make adequate

¹ *Twenty-third Report of the Commissioners in Lunacy to the Lord Chancellor.* Ordered by the House of Commons to be printed, July, 1869, pp. 408.

Eleventh Annual Report of the General Board of Commissioners in Lunacy for Scotland. Presented to both Houses of Parliament, pp. 310. Edinburgh, 1869.

provision for their insane poor. With this wide subject, however, we cannot at this moment fully deal, but propose to examine the contents of the two reports now before us with especial reference to the growth of insanity in this country.

In our article on "Lunacy in England," published in this Review in April, 1869 (p. 383), we remarked on the defective returns of the state of lunacy made by the English Commissioners; we are glad, therefore, to find in the present report a considerable improvement in this matter. The collected statistics for the past ten years offer valuable data respecting the increase of lunatics in institutions under the more immediate supervision of the Commissioners, *i. e.* in all receptacles for the insane, exclusive of workhouses, and of lodgings in which outdoor paupers are distributed.

On the first of January, 1869, the total number of lunatics known to the Commissioners was 53,402. This is inclusive of pauper insane placed in workhouses and boarded out singly, and also "of 225 lunatics, so found by inquisition, and residing with their committees." The entire increase for the year was 2,177, and was thus distributed:—in the class of private patients, 122; in that of paupers, 2020; and of criminals in Broadmoor, 35. This enormous preponderance in the increase of pauper compared with private patients in asylums is more decidedly displayed by a table furnished by the Scottish Commissioners, which shows that, in England, in the course of the five years ending January 1st, 1867, among the former the total addition was 5049, and among the latter only 36. On this fact the Commissioners observe:—

"The population of England may be estimated in round numbers at twenty millions, of which one million are paupers; and we have thus the remarkable fact of the increase in the number of asylum patients in five years of 5049 from the million of paupers, and of only 36 from the nineteen million of non-paupers. Making very liberal allowance for the pauperising effect of lunacy, and the consequent removal of a very considerable number of patients from the independent to the pauper class, we are thus forced to the conclusion that insanity is essentially a disease, not of the overstrained intellectual or emotional faculties, but of the depraved bodily condition, which, for the most part, is dependent on insufficient or inappropriate food, irregular living, overcrowded dwellings, long-continued nursing, overwork, fever, or any similar cause of bodily debility" (p. vi).

Without challenging the general inference as to the common cause of insanity, we must object to the calculation that the vast increase noted took place among the theoretically assumed one million of paupers. Much above a million individuals are enu-

merated in the official lists of paupers; but, apart from this circumstance, if we examine the tables of occupations, and the particulars furnished in the reports of county asylums of the patients admitted into such establishments, we shall be struck with the smallness of the number of those belonging to the class of paupers proper figuring in Poor Law Board lists, and reckoned by the million.

In Scotland, "on an average of ten years, of every 100 patients sent to asylums, 27·1 were private and 72·9 paupers."

The decennial table presented in the English Commissioners' report, shows that, between the 1st of January, 1859, and the 1st of January, 1869, there was a total increase of registered lunatics of 16,415; and that, whilst only 840 private patients were comprised in this grand total, 15,114 were entered as paupers. To quote the words of the report:—

"It thus appears that the number of lunatics, as far as our official records extend, has increased 45 per cent., during the decennial period ending the first of January, 1869, and that 67 per cent. of this increase is attributable to additional pauper patients in county and borough asylums" (p. 4).

There has also been an actual advance in the ratio of lunacy to the population; for whereas in 1859 there was 1 known lunatic in every 536 persons then alive, it now appears that there is 1 such individual among every 411.

During the decennium "the estimated population increased rather more than 11 per cent., whilst the total number of recorded lunatics has increased to 5 per cent." (p. 5).

In the course of the same ten years the increase in Scotland has been from 6044 to 6976 (932 in all), which represents an advancing ratio to the population of that country of from 1 in every 496 inhabitants to 1 in every 487, assuming the population to have advanced only 10 per cent. But if a calculation were made of an increase of 11 per cent., as is done for England, the result would be still more in favour of Scotland.

The great public institutions for the insane, comprising the county and borough asylums and the registered hospitals, contained, on the 1st of January, 1868, 27,961 lunatic inmates out of the whole number of 33,487, returned as resident in special establishments. The number detained in public asylums had, in the ten years, increased by 9,836; but the residue found in licensed houses was somewhat less at the end of that period.

It is a melancholy fact that, with all the enlarged and improved provision made for the insane year by year for many years past, there is no decided advance in the ratio of recoveries, as calculated on the admissions. The average for 10 years in

county and borough asylums was only 25·53 per cent., and that in hospitals 40 per cent. In Scotland it appears to have equalled 36·51 per cent. in the district asylums.

Further, the death-rate in Scottish public asylums, as calculated on the average number resident, is considerably smaller than that found in the like English institutions, amounting, in the former, to 8·09, and in the latter to 10·83, or, by grouping registered lunatic hospitals with the county asylums, 9·15. This difference in the death-rate in the two countries is principally due to the greater mortality prevalent among male lunatics in English than in the Scottish institutions.

The facts disclosed in Table IX of the distribution of pauper lunatics upon the 1st of January in each year, for eleven years, present a most discouraging comment upon the hopes and aims of the promoters of asylums, and suggest doubts of the gravest character as to the present policy of dealing with the insane. The time was when Commissioners in Lunacy and others cherished the hope that when every county possessed a fitting asylum lunatics would no longer be retained in workhouses. These places were held, and most justly so, improper receptacles for such disordered folk, and only to be put up with temporarily until the projected asylums were erected. The constant aim was to empty the workhouses of their insane inmates, and to transfer these to the county asylum; and ever and anon congratulations were heard that this workhouse-“delivery” had been here or there effected; but if we accept the figures of the table in question, all these bright hopes and humanitarian projects have been cruelly sacrificed by the unlooked-for rate of accumulation of chronic lunacy. On inspecting the table we observe that pauper lunatics have augmented, during the ten years 1860-69, 14,000 in round numbers; and that, notwithstanding asylum accommodation has in the same period been enlarged by very nearly two thirds, yet the lunatics detained in workhouses in 1869 were close upon 3,000 more than in 1860. At the same time the pauper insane distributed in lodgings have advanced from 5980 to 6987. To quote the per-centage calculated on the total number of recorded lunatics, as given in the table; whereas 59·96 per cent. of that number were accommodated in asylums in 1860, 61·26 were so placed in 1869; whence it appears that, although asylum provision advanced 70 per cent. in the ten years, only 4·30 more lunatics obtained the benefit of it. In other words, the asylums were encumbered by accumulation amounting to 65·70 per cent.; and whereas in 1860 the per-centage of lunatics in workhouses was 24·91 and in 1861, 23·84, the reduction in their number has reached only 1·07 per cent., an almost inappreciable result, and one which, with the

evidence of the progressively increasing proportion of insane in workhouses, as furnished in the present report, may be altogether ignored.

The practical conclusion is, that the immense additions made to asylum accommodation during the last ten years, have been fruitless, so far as they were intended as means of bringing all pauper lunatics under proper supervision and under the protection of the lunacy laws—a privilege not belonging to workhouse inmates.

The bulk of the English Commissioners' report is taken up with a summary of their observations, expressed opinions, and official dealings with respect to the various public and private asylums of the country, and is accompanied by a copious appendix, consisting of the entries made by them at the date of their visits to the several institutions. This appendix of "entries," doubtless, addresses itself forcibly to the superintendents in charge of the several asylums, particularly when the notes are of a character flattering, or the contrary, to their management; but it will fail to secure the attention of the inquirer into the general state of lunacy in the kingdom, by reason of the diffuseness of the information it contains, although, indeed, capable of furnishing by careful analysis important contributions towards a right apprehension of that question. This capability of yielding noteworthy results was shown in our review of the previous report ('Review,' April, 1869) where we undertook an examination of the prevailing practice in English asylums, in the matter of mechanical restraint and seclusion. On this occasion we have decided to take up, as before stated, some of the larger questions suggested by the analysis of the statistics of lunacy contained in the present report. In dealing with these we shall derive great assistance from the report of the Scotch Commissioners.

The following facts may be assumed as established by the official returns under notice:—1. That there is a progressively increasing ratio of lunatics to the whole population. 2. That, in the course of ten years, the number of recorded lunatics has increased 45 per cent.; 3. That two thirds of this growth of lunacy has taken place among paupers; 4. That there is an enormous accumulation of chronic lunacy; and, 5. That the proportion of lunatics detained in workhouses continues undiminished, notwithstanding the augmentation of asylum accommodation seventy per cent. in the course of the last ten years.

This being so, the inference forces itself upon the mind that the present system of providing for lunatics is a failure, when viewed as a means not only of securing safety to the unfortu-

nate insane and to the public, with careful tending and good living for the former, but also of controlling the growth of insanity by promoting its cure.

There has been much sensational writing respecting the increase of lunacy in the country, and the public mind is strongly impressed with it as a painful and perplexing fact; but the circumstance of mere accumulation has been greatly overlooked by popular writers. On the other hand, ingenious attempts have been made by recourse to statistics—so facile, whether in the cause of truth or of error—to demonstrate that, regard being had to the growth of the population and to the results of accumulation, no positive augmented production of insanity has taken place, but that, on the contrary, we are not now so mad a people as we were some years ago. This comfortable conclusion, however, does not derive support from the inquiries of the English Commissioners; although, indeed, it is not so far from the truth as that of the alarmists. To quote the ‘Report’ on this matter, the Commissioners remark:—“The ratio of admissions of certified insane patients to the population appears to have been 1 to 2115 in 1859, and 1 to 1931 in 1868; and the average annual increase of the admissions appears to have been about 2·04 per cent., the average yearly increase of the population being estimated at about 1·11 per cent.” (p. 7).

This fact indisputably points to an absolutely increased production of lunacy in the community;—a positive growth, distinct from augmentation resulting from accumulation. At the same time accumulation is chiefly chargeable with the large relative increase, as above quoted, of 45 per cent. among lunatics in the course of the past ten years.

The causes that have operated in the multiplication of lunatics, particularly in the public pauper institutions of the country,—where the growth is almost special and peculiar,—are of a mixed character, and traceable to conditions within as well as without those establishments. We cannot now examine all that may be enumerated, but must confine our attention to some that more prominently present themselves to our mind. The first cause we shall adduce is, the physical, moral, and social condition of our labouring classes. This condition we regard as one of physical and mental degenerescence with respect to no small numbers. Physicians conversant with the maladies of the working classes, both in town and country, but especially in the former, cannot fail to be struck with the increasing prevalence of hereditary diseases, foremost among which stand scrofula and pulmonary consumption. But besides inherited proclivity to disease, those classes suffer from the surrounding circumstances of life affecting their health and morals; the more so the resi-

dents in large cities. The growth and exigencies of manufacture have brought about aggregations of people, often pretty exclusively composed, in many town districts, of the labouring class. The close grouping of people, sanitarians will tell us, is inimical to their physical well-being; and moralists will further advise us is also detrimental to their moral health, particularly when they are of one class, and so cut off from the contact, the moral control, and the influence of the wealthier and better educated orders, and left to work out their own moral development. The tendency of such aggregations, left to themselves, is to degenerate in mind and body. Moreover, there are many influences at work in modern society that contribute to widen the breach between capital and labour, intellect and handicraft. There is besides a recklessness among artisans, not in exceptional cases, as must ever have happened, but in a large proportion of them. Their elevation in political power and importance, and the immense rise in wages of all sorts, without any proportionate advance in education and intelligence, the preaching of social equality and privileges by demagogues, the laws and proceedings of trades-unions, the growth of much indiscreet philanthropy offering a premium to idleness, and the very provision itself made by the State for relieving pauperism (in so far as it offers an asylum in age and broken health to all soliciting it as paupers); these, and probably other circumstances, have conspired to bring about the recklessness of which we have spoken, and a lamentable indifference to the future. This condition is shown by the wasteful expenditure of wages in self-indulgence, and particularly in intoxicating drinks, and in a prevailing indisposition to regular labour.

It would, in this article, be out of place to enlarge upon these causes of social and physical degeneration, and to show in what directions they are operative; but it requires no elaborate argument to prove that habits of intemperance, of self-indulgence in animal pleasures of all sorts, and of indolence coupled with cloudy notions of independence, and with a certain animosity towards the well-to-do and educated members of society, must brutalise the mind and render it a more easy prey to disease.

But further, the vicious conditions referred to operate injuriously to the physical and moral wellbeing in many indirect ways. The intemperance and excessive indulgence of one day have to be paid for by the want and depression of another; the indulgence in drink often entails a deficiency in quality if not in the quantity of solid food—a deficiency felt especially by the wives and children of the men. Dr. Edward Smith noted some years since the low, insufficient diet common among artisans in manufacturing towns, and those conversant with the lower

ranks of the labouring classes know how inadequate is the food consumed by a considerable proportion of them, and to what enfeeblement and premature old age this circumstance brings them. We might readily illustrate these facts, but it would lead us far too much away from the subject in hand. It is, however, sufficiently patent to every reflecting mind that the irregular living, and the depressing and debilitating consequences of such living, the misery entailed by it in reference to the social relations of life, are, as above noticed, causes of physical and moral degenerescence in the individuals themselves subjected to them, but still more, as must be added, in the children begotten of them.

We have seen that the Scottish Commissioners have, and as we believe erroneously, assumed that, as the increase of lunacy is pretty exclusively met with in district and county asylums, established for the reception of paupers,—that is, of those chargeable to parishes as paupers,—this increase is nearly exclusively due to the prevalence of insanity among the labouring classes reckoned by the Poor-Law Board in their lists of paupers, *i. e.*, people in the receipt of parochial relief. Nevertheless, there is no doubt that the class next above this truly pauperised one, contributes a large contingent to the ever-augmenting insane population of the country. It is a class composed of agricultural labourers, of mechanics, of servants and ill-paid female artisans, who are at once thrown upon the public assistance when insanity befalls them. Yet there is a somewhat higher class that finds its way into pauper asylums, and which adds a no inconsiderable quatum to the total insanity, derived from small shopkeepers and masters in various trades, from governesses and unsuccessful professional men. In this class other causes are operative than those sketched as prevailing among the working classes. Intemperance and other vices may make victims to insanity among them; but they are more exposed to social and moral causes, to over-anxiety and over-mental strain consequent upon the rivalry in trade, on failure in business, or on jealousies and disappointed hopes. Judging, indeed, from the statistical tables of asylums setting forth the social position, the occupations and the assigned causes of disease of those admitted as patients, the proportion of lunatics derived from this lower middle class is, particularly when viewed in connexion with the relation it bears in point of number to the artisan and labouring class below it, exceedingly large, probably equal to that coming from all other classes represented in county pauper establishments.

The report of the Commissioners for Scotland refers, at several places, to the increase of lunacy and to its causes. The

proportion of pauper lunatics differs much in different counties ; a circumstance that—to quote the report—“ must be dependent on differences in the constitution of the inhabitants, on differences in their education and mental culture, on different degrees of social intercourse, and in the amount and nature of their occupations, and on differences in their pecuniary position. As a rule, however, it may be assumed that there is a greater degree of mental activity among the urban and manufacturing population than among one which is chiefly agricultural, and to this fact may possibly be ascribed the more frequent *occurrence* of insanity among the former Without calling in question that over-excitement of the intellect or feelings is a not infrequent cause of insanity, we are nevertheless satisfied that among the pauper lunatics who are admitted into asylums, the cases which originate in this source are few in comparison with those which are due to physical deterioration.” Since the development of the asylum system (the commissioners proceed to observe), “ the condition of the lower classes has suffered from their increased accumulation in towns, and from the prevailing ignorance of the population of hygienic laws” (pp. 15, 16).

The influence of general wretchedness, of poverty and insufficient food, and in some degree also, probably, of unusual isolation, in developing insanity, may be gathered from the report of Dr. Mitchell on lunacy in Shetland (p. 297), as likewise from the excellent essay on insanity in Arctic countries, from the pen of Dr. Lauder Lindsay, published in the number of this Review for January last (1870).

Passing now from the consideration of general causes of the increase of insanity discoverable in the population at large, let us glance at some others to be found in our public asylums. It must be premised, however, that these are causes of accumulation and not actually productive of insanity. Of such causes some are concerned with the admission of patients, others with the management and treatment of the inmates of asylums. Our notice of them must be very brief and unaccompanied by the mass of evidence that might be advanced in elucidation of their operation.

Foremost among causes associated with the circumstances of admission of lunatics into asylums for the purpose of treatment, is the prevailing neglect in sending them in the early stage of their malady. This neglect is traceable to the prejudices of private persons against consigning their afflicted relatives to an asylum ; on the part of poor-law officers, to mistaken notions of the superior economy of workhouse wards ; and to the fact that the county establishment is ever and anon so overcrowded that it cannot receive additional inmates.

It has, in the second place, always been found that the erection of an asylum creates, so to speak, a supply of inhabitants, and from the very first becomes stocked with chronic cases from the workhouses and cottages of the surrounding district. The number of such cases universally proves far in excess of any estimate of them previously made, and consequently, in a short time, the accommodation provided according to the official returns supplied is found inadequate. It is an important problem how far this gathering together of all the halt and maimed in mind within an expensively constituted and organised building is necessary and judicious. Weak brains must ever abound in a civilised community, and it will remain impossible, not to say most inexpedient, to attempt to make provision for the care and tending of all defective and aberrant intellects. The practical question is, What shall be the criterion to employ in judging of the necessity of segregation? Physicians charged with the care of lunatics exhibit many crotchets, but no appreciable principles in determining this question. Indeed, their notions about it seem very hazy, and their usual practice to be a quiet acquiescence in the expediency of receiving as inmates those sent to them. At the same time, it must be admitted, that their powers of rejection are well nigh null, when the requisite order and certificate in due form, setting forth some particular statements, called facts indicative of lunacy, are put into their hands, but orders and certificates may be worthless as affording a criterion of the necessity for the asylum treatment sought. The orders may come from parochial officers who want to be rid of a troublesome individual, and the medical certificate from no expert in lunacy, and one holding a parochial appointment, who honestly recognises that the pauper admitted to his examination is more or less crazed, and will be better off in the asylum than in the workhouse ward, and who, in fine, has no inducement to oppose the proposed transfer.

The gathering of chronic pauper insane from the general population, and their perpetual retention within asylums is of questionable propriety, with at least a portion of them, when viewed in connexion with their personal welfare and their relation to their parents and kindred. It breaks asunder those natural ties and sympathies which should bind members of a family together, and which experience tells us are, as a rule, rendered stronger by the occurrence of sickness and misfortune. All must admit sickness to be an evil, yet it is one that has its compensation in arousing sympathy and kind feeling, in provoking to good works and even in strengthening affection; and it is no uncommon thing to see a weakminded child the pet of the family. Without doubt, the presence of an imbecile in a household involves

trouble, cost, and anxiety ; nevertheless, the exile of the afflicted member is not always desirable on that account even for the well-being of the family, unless, indeed, the home is one of abject poverty and moral disorder. The separation is apt to entail indifference and callousness, and the facility of transferring weak-minded relatives to asylum wards will discourage personal effort and self-reliance, as well as weaken the virtue of moral sympathy and support.

At the same time, the ill effects of separation upon the afflicted member of the family are more appreciable than on the relatives and friends. Any sympathy or affection he may have had towards his kindred is broken through ; he is merged among strangers ; his individuality is lost sight of amid the multitude of other such sick folk in the asylum ; the whole tenor and mode of his life is changed, and he is subjected to rules, restrictions and routine more or less wearisome and offensive to him, and, to complete the picture, is tended by a corps of expensive servants and officers and lodged within a costly building, planned and arranged with a view of providing for a class of mentally disordered beings to which possibly he does not belong.

The English Lunacy Commissioners do not exhibit any vivid interest in this matter of the condition and requirements of the chronic, helpless, and imbecile insane that encumber asylums and cause their doors to be closed to recent and curable cases. Now and then, under the pressure of circumstances, they apply to superintendents of asylums for returns of inmates, who may be sent out as harmless and miscible with the occupants of workhouse wards ; but they do not embark on the question of dealing with the mass of chronic insanity, in its wide aspect, with regard to cases without as well as within the walls of asylums. They show by statistical tables that the proportion per cent. of lunatics boarded with relatives or others, compared with that provided for in asylums and workhouses is slightly decreasing, and with this result they would seem satisfied. But, on the other hand, they stand rather aghast at the growth of asylums and of workhouse lunatic wards ; and, what is worse, they appear helpless in suggesting a remedy or even a palliative. They would seem to be committed to the one plan of herding lunatics within huge establishments, notwithstanding that all experience proves the rottenness of that plan.

It is well known that, in Scotland, the transfer of cases of lunacy of all sorts to asylums or lunatic wards is discouraged, and the welfare of single cases lodged in cottage homes provided for by a system of registration and visitation by assistant commissioners. In illustration of the working and application of this arrangement, we will quote a few paragraphs from the

report of Dr. A. Mitchell, one of the assistant commissioners. During 1868, he visited in his own district 433 single patients so boarded out (p. 292) :

“ The reports show the condition of many of the registered single pauper patients to be quite satisfactory ; in other words, it has been found that a fair provision has been made for the safety and well-being of a considerable majority of them, making removal either to an asylum or to the lunatic wards of a poorhouse unnecessary and undesirable. This accords with the experience of past years. Nothing, indeed, has been seen to lead to any change in the opinion expressed in former reports as to the possibility of providing satisfactorily for a certain number of the insane poor in private dwellings. The happiness of many patients is best promoted by allowing them to live in family. There is nothing in their condition which calls for the machinery and discipline of an asylum. They are incurable and harmless ; they require no *special* medical care, and the freedom of home life is enjoyed by them, and proves beneficial to their health. It would be difficult in such circumstances to assign reasons for removal to an asylum which would be sufficient. * * * By careful selection and by judicious and liberal arrangements it is reasonably hoped that the proportion (which actually is increasing year by year) —of such single patients will be still further increased. The object is to retain in private dwellings none but suitable cases, and to secure for these a proper amount of comfort and care. That this can be done for some patients does not appear to be anywhere doubted. There are persons of unsound mind found in private dwellings whose removal to asylums I scarcely think any one would declare to be desirable. This being so of *some*, the question is, whether it cannot be made so of *more* ? and I think experience has shown that efforts in this direction may succeed, and that under proper regulations, the condition of insane poor in private dwellings may be greatly ameliorated” (p. 282).

At p. 300 Dr. Mitchell details the instructions issued to inspectors of the poor, which his experience has shown to be necessary for securing as much attention and comfort as possible for this class of single patients. His colleague, Dr. Paterson, in a few words, confirms the views advanced in the above quotation, observing that his prolonged experience proves the opinion he has been in the habit of expressing, “ that many cases of a chronic and harmless character, whether of natural or acquired insanity, are equally well, if not better placed in family life than in establishments” (p. 301-302).

There is one remark in Dr. Mitchell’s observations above quoted, viz. :—that this mode of distribution of lunatics in families “ proves beneficial to their health,”—which is amply confirmed by the tables of mortality printed in the general report of the Scottish Commissioners (p. 9 and p. 35). From these tables it

appears that the average mortality of patients for a period of six years, calculated upon the average numbers resident, was, in English asylums, 10·39 per cent., and in Scottish 8·45 per cent., whilst that of pauper lunatics in private dwellings was, for an average of eight years, on the number registered in each year, only 5·6 per cent. In Scotch parochial asylums and lunatic wards of poorhouses the mortality has averaged 11 per cent., and thus has been in excess of that in the asylums.

The following pertinent remarks are made by the Scottish Commissioners, on a review of the mortality tables :

“The mortality among pauper patients in private dwellings is thus seen to be more favorable than among patients in establishments. That it should be less than the mortality among asylum patients is not surprising, considering the amount of active disease in such establishments; but that it should be so considerably less than what occurs in lunatic wards of poorhouses must appear remarkable, especially when it is taken into account that the patients in such wards are, for the most part, like those in private dwellings, idiots and demented, and that, as a rule, the physical wants of the former are more amply supplied. We can offer no explanation of this fact beyond the conjecture that the manner of living in private dwellings, involving, as a rule, greater freedom and greater variety, and the respiration of an atmosphere less loaded with animal exhalations, more than counterbalances the advantages which better diet, better clothing, better bedding, better housing, and greater cleanliness, might be supposed to convey” (p. 35).

Another feature in this plan of boarding out single patients, and one that must commend it to every practical economist, is its small cost. The average weekly cost of each patient in a public asylum of England or Scotland is about nine shillings and sixpence, and in workhouses seven and sixpence; whereas that of patients in private dwellings in Scotland is only three shillings and ninepence.

Another circumstance that exercises a wide influence in causing an accumulation of the insane in asylums is, the over-estimate formed of the need and importance of such institutions for the insane at large. In bygone times the insane were regarded with superstition and dread, and dire imprisonment was in consequence their lot. In recent years something of this awe of insanity has been dispelled by the humanising influence of our modern asylums; but the passion for sequestering its sufferers has not therewith abated, owing to the doctrines advanced of the necessity for securing due protection and care for them of a sort held as not attainable in private dwellings. Hence, as above noticed, there has been an indiscriminate sweeping together into asylums of every idiot, imbecile, and mentally odd

individual who has obtruded himself upon the notice of parochial authorities, or has become burthensome and unpleasantly costly to his economically minded relatives and guardians.

The Scottish Commissioners show themselves fully alive to this important circumstance. They consider the detention of patients in asylums under "these three following aspects:—1, in relation to the patients; 2, in relation to the asylums; and 3, in relation to the parties who defray the cost of maintenance." The first aspect under which detention is viewed refers to the value and necessity of asylum treatment. We should like to have copied the whole of their remarks on this point, but our space forbids, and compels us to curtail them.

"It is generally admitted [say they, p. 39] that a certain proportion of the insane may very properly be placed in private dwellings. After two or three years' asylum treatment the chance of recovery becomes so small, that the inducement to continue detention on this account may be said to cease. The inducements which remain for consideration are the risk of violence, either to the public or to the patient himself by removing him, and the possibility of his suffering, from being transferred to circumstances less conducive to his comfort and happiness. Practically this last point is the only one to be considered. Now, "no doubt the bedding, clothing, and diet of asylums will usually be better than those to which the peasant or the labourer has been accustomed at home."

But as a set-off against such advantages the patient in a private dwelling enjoys a more natural and congenial existence, and, as the tables of mortality show, his chances of life are better than in asylums or in workhouse wards.

"It is a common argument in favour of asylum treatment, that the patients are under the constant supervision of experienced medical men, and the persistent care of specially trained attendants. These advantages are unquestionable; nevertheless, in many cases of chronic insanity, there is in reality less necessity for constant supervision than in such diseases as consumption or dropsy. Indeed, for a very large proportion of the chronic insane, nothing more is required from the attendants than ordinary tact and kindness, which will perhaps be found more readily in the cottage than in the asylum. To suppose that by mere transference to the wards of an asylum, a ploughman, policeman, discharged butler, or mechanic or artisan out of work, is to become a devoted attendant on the insane, is simply to expect impossibilities. Nor, on the other hand, is it much to be feared that the kindness and humane treatment, which, in the cottage of the ploughman, or the dwelling of the artisan, are extended to the sufferers from consumption, paralysis, or blindness, would fail in the cases of those affected with chronic insanity. And in favour of

cottage treatment there is this great advantage, that male patients are not, as in asylums, deprived of the watchful care and tenderness of female nurses" (p. 39).

We are persuaded that these observations of the Commissioners for Scotland will not fail to have their due weight in the future discussion that must ensue on the great social question of what to do with our lunatics. Their views in regard to the third aspect of asylum detention, viz., "in relation to the parties who defray the cost of maintenance," equally deserve notice. The problem presented is how, with an ever-increasing growth of lunacy, to save further pressure upon the rate-payers. The commissioners hold that, "when once a district has provided an asylum sufficient for the proper care of all the insane who are unable, or who, from the nature of their malady, require to be detained, . . . the provision of further accommodation becomes an injustice to the rate-payer;" and they recommend that, instead of opening the wards to all applicants and retaining the unrecovered for the term of their lives, and consequently calling for repeated extensions to meet the accumulation of chronic cases, it would be the proper course "to require the removal of patients after a certain period of treatment," under proper certificates as to their incurability and the absence of danger from them. To quote from another page, (41)—

"That the removal of hopelessly demented patients would prove a boon to asylums is undoubted. Ordinary care and attention is all that they require; and if precautions were taken to remove only patients in whom active disease had ceased, there is no reason to fear that any evil consequences would ensue; . . . and if superintendents of asylums would regard it as an essential part of their duty to co-operate with the parochial medical officers for the removal of cases which are clearly beyond the hope of curative treatment, and are otherwise not unsuitable for disposal in poor-houses or cottages, the interests of the rate-payers would receive legitimate consideration without injustice to the insane poor."

Economical considerations on the part of the less wealthy middle classes also lead to the filling asylums with chronic cases. This is shown by the official returns of the existing numbers, and of the ratio of discharges of patients in private asylums. The number of inmates of these private establishments on the whole annually decreases, whilst the ratio of the discharges from them of uncured is very high. The Scottish Commissioners have compiled a table of discharges from English public and private asylums for a period of six years, and the fact they gather from it is, that, "although the patients in county and borough asylums are three times as numerous as those in hospitals and licensed houses, yet the discharge of unrecovered patients from

the latter class of establishments is considerably the greater, and there can be no doubt that this arises from the preponderating number of private patients which these contain" (p. 5).

The inducement for such removals is the saving of the expense of maintaining their friends in the private establishments; and the same inducement is operative in sending, in the first instance, also, many persons, far removed from pauperism, to the county asylums. There is much to be said in explanation and in extenuation of this practice, though doubtless there is much abuse in it. Those competent to maintain friends in an acute illness for a short time, will often be wanting in means to continue payment for a long series of years; and with respect to the transmission of well-to-do-people soon after their attack to county institutions, it may be urged that a good end is attained by securing for them prompt treatment. The fault is the want of some legally recognised plan for such transactions, and the abolition of private arrangements made by the intervention of parish officers, and the possible imposition upon the county funds and peculation.

These remarks and quotations must suffice, for the present, to illustrate the causes outside asylums tending to the growth of insanity, and especially by accumulation. To follow out the other division of our examination, we have to consider those causes in operation in the organization of asylums and in the treatment pursued in them. To fully consider these matters would lead us beyond all due bounds prescribed for a review; we shall consequently limit ourselves to some general observations on accumulation in asylums. This accumulation, as already noticed, follows on the exaggerated and mistaken notions respecting the value and necessity of asylum treatment for lunatics generally, on the indiscriminate admission of cases, and on the continued detention of patients in whom recovery is hopeless and special treatment unnecessary.

The very character itself of a pauper county asylum leads to its abuse. It undertakes the care and treatment of a patient gratuitously. To quote the Scottish Report, "When the cost of maintenance is defrayed by the parish, the pecuniary motive to removal ceases to operate; and as the family of the patient, as well as the inspector of the poor, are relieved of all trouble and responsibility in connection with the case as long as the patient remains in the asylum, a considerable inducement is, on the contrary, held out to leave him there" (p. 5).

Nor can the superintendents of asylums be acquitted of at least a passive acquiescence in this state of things. Witness the following notice of the Scottish Commissioners' experience:

—“The statutory provision which requires the certificate of the medical superintendent for the prolonged detention of patients, after the expiry of three years from the date of their admission, has not led to the discharge of a single case during the three years it has been in operation ” (p. 41).

Several causes of this unreadiness of medical superintendents to recommend the discharge of chronic cases may be assigned. In some measure they may be unwilling to take all the onus and responsibility of the discharge on themselves. They have too many masters and cannot please all. They are amenable to commissioners and committees; their acts are exposed to the criticisms and objections of parochial officers and self-opinionated vestries; they may themselves be dissatisfied with the arrangements made for the transfer of patients; and, on the other hand, may encounter the opposition of parish boards to the proposed discharge, and further be convinced that, should such authorities yield to their recommendation, they may assuredly reckon on its being rendered abortive. Another impediment to their heartily falling in with the proposition to discharge incurables may be found in their misgivings as to the fitness of cases. In large asylums, it is a misfortune that superintendents have to rely largely upon second-hand observation and information. The individuality of patients is merged in the general mass, and the medical officer has to trust in a great measure to the reports and opinions of his attendants respecting the condition of individual patients. In the overgrown receptacles for the insane the resident medical man is an overseer, steward, and general manager; a surgical aid at hand in case of accident, and a visiting physician in case of disease; but not by possibility an explorer of individual peculiarities, or fitted by personal observation to decide on the propriety of discharge with that assurance as shall give him confidence in his recommending it—excepting, indeed, that amount of it derived from reliance upon his subordinates.

Once a patient always a patient, appears to be the maxim in the case of chronic lunatics who have found their way into a pauper asylum. The organisation, administration, and prevailing opinions and practice relative to the insane, have concurred in converting what should be hospitals for the cure of insanity into receptacles for all forms of aberration of the human intellect, acute and chronic, imbecility and idiocy, in which these several forms are more or less jumbled together and subjected to peculiarly faultless routine and carefully planned rules for order and cleanliness, and the whole conduct of life.

The patients are comfortably housed, their character and peculiarities are sufficiently well known to those in charge of

them to render their supervision and management easy ; they have work and amusements provided for them ; they, on the whole, give little trouble. They are acquainted with the internal economy of the establishment, and have become, as it were, integral portions of the vast machine—members of a happy family. Why, then (we may imagine the argument), disturb them and cast them adrift among strangers? Why destroy the pleasant harmony by displacing the old inhabitants for so many new, who may, by their irregularities and improprieties, jar the even working of the establishment, introduce discord, and give rise to a world of trouble to all its staff? Surely in this argument there is much weight, if only, indeed, it is proposed to collect lunatics together in safe and comfortable abodes. It is an argument for a *laissez faire* course of action among those concerned in the management of pauper asylums.

The accumulation of chronic lunatics and overcrowding in asylums have proceeded to such lengths that their evils are now universally recognised. Accumulation has led to extension upon extension to asylums, until these buildings have become really unmanageable considered as curative institutions ; indeed, they are inimical to recovery. They represent vast communities of insane, subjected mostly to lifelong imprisonment under the show of being patients submitted to medical care and treatment. The medical element figures in the scheme of their organisation, but it is reduced to a minimum, and ceases to be practically of much importance ; and, in some institutions, is so subordinated to the general administration, that in all but its usefulness as a safeguard against troublesome incidents, it might wellnigh be dispensed with altogether.

But some may exclaim, that it is the moral treatment that is all-important, and that the medical is truly and properly subordinate. Moral treatment is assuredly of prime importance for the insane, but it is a term that may be used in apology for the want of most of the essential requirements of a place of cure for lunatics, and imply little more than a well-organised routine calculated to bring down all the minds subjected to it to the same dead level of dementia.

“What—(write the Scottish Commissioners in reference to this matter of merely aggregating the insane in a common establishment and under general discipline)—what beneficial results, either to mind or body, are to be expected from the mere agglomeration of a number of insane persons in a common ward, or from their being turned out to lounge together in a common airing-court? If asylum treatment have no higher aspirations than these, it is certainly time that the whole question of the management of the insane were thoroughly reconsidered” (p. 40).

The Middlesex asylums stand foremost as examples of the evils of accumulation and of overcrowding, and the English Commissioners have made strong representations to the Lord Chancellor upon their unsatisfactory state. They, moreover, may be taken as samples of what other county asylums threaten to become under the present system of dealing with our lunatics. Although Hanwell and Colney Hatch Asylums have, by repeated enlargements, been rendered capable of containing, the former an average population of 1710 lunatics, and the latter of 2050, yet the Commissioners make it appear that "nearly 600 Middlesex patients were, in the course of less than one year, turned from the doors of *one* of its asylums alone" (p. 19); and they press upon the magistracy of the county the urgent need to erect, without delay, a third asylum, to accommodate 1000 patients. They justly complain that recent cases of insanity cannot obtain admission, and are consequently deprived of those chances of recovery that asylum treatment holds out; and also that, in consequence of the same want of accommodation, very many Middlesex patients are permanently lodged in workhouses, and many transported to asylums at a long distance from their homes, contrary to the intention of the Legislature, "that they should be so placed as not to be deprived of the possibility of being visited by their friends" (p. 28).

Surely the history of these metropolitan asylums should serve as a warning to the authorities in other counties against pursuing a like system of herding lunatics in overgrown establishments. It offers an illustration, not to be gainsaid, of the failure of the system, and we should have been pleased to have seen the Commissioners, instead of pursuing the same track by recommending a third establishment for a thousand patients (to be inevitably followed by a fourth within a few years), raise the whole question of providing for our lunatics, and, armed with the experience they possess, discuss all possible expedients for arresting this most astounding and embarrassing accumulation.

It is clear to us that the first remedial step to be taken is to provide for the curative treatment of recent cases, and to bring such cases under treatment as soon as possible. It certainly cannot be promotive of recovery to introduce a recently attacked patient into a ward full of chronic incurables, where the organization and management are directed to the wants of those incurables, and the attendants are naturally in accord with those wants and with a customary routine. It cannot afford the patient the best chance of curative treatment to be mingled in a community of chronic lunatics, to be one unit only in a thousand demanding the supervision and care of the medical officer, charged as that officer commonly is with multifarious non-medical duties

and responsibilities. The mutual relation and co-adjustments of the several parts of a complex machine must not be disturbed by the introduction of a new portion, but the novel parts must be subordinate to, and regulated by, them. The recent case must become as one of the chronic, and take its share of supervision and treatment in common with them, according to the regulations and routine in force.

There is, therefore, good reason for the existence of such small institutions for recent cases of lunacy as St. Luke's Hospital, London. The statistics of this hospital show what may be done in the way of curing mental disorder, even when some of the external circumstances deemed desirable, if not essential, to an establishment for treating the insane are wanting. It shows the direction our endeavours must take if we wish to diminish the accumulation of chronic insanity. Nevertheless, it is the best abused establishment for lunatics in the kingdom. Besides anonymous detractors in the public press, it unhappily has persistent opponents in the Commissioners in Lunacy. Time after time it is the subject of special reports by them animadverting upon its defects; and in the present (the twenty-third) Report, they take occasion to give a summary of the comments they have made during the last twenty years. Besides objections advanced against certain points in its administration which ought to be readily overcome, they protest more especially against its site and its construction, and argue that in these particulars it is entirely unsuited to the treatment of lunatics. And, in truth, it must be granted that there is much to be deprecated in its architectural features, in some of its internal arrangements and likewise in its locality.

Notwithstanding it was, at one time, a model hospital for lunatics, and several asylums were constructed after its general plan, and almost every asylum in the country has so far copied it as to adopt the corridor system of construction, with this difference only, that they have not reproduced equally spacious corridors and equally large single sleeping-rooms. The fitness of its site is, however, the more important question, for in its internal arrangements for the convenience and comfort of the inmates it is on a par with, if it do not excel, not a few county asylums. The site has certainly many disadvantages; it is in a poor and thickly peopled locality and its airing courts, besides being very limited, are partly overlooked by neighbouring houses. But, leaving out of view these accidental defects of its town site, we are not prepared to admit the dictum of the Commissioners that a town site is, *per se*, unfit for a lunatic hospital. Such an hospital within the boundaries of the colossal metropolis offers advantages in facilitating the removal and treat-

ment of acute cases, and in supplying a school for the clinical study of mental disorder. The benevolent founders of the hospital put the latter object forward as a leading argument for its establishment. Again, the principle of an urban hospital for the immediate reception of cases has been adopted in accordance with the views of the most eminent psychological physicians in France, and a central institution has recently been erected in Paris.

In discussing the suitability of site for an hospital the matter of convenience and accessibility cannot be ignored, and in the case of a lunatic hospital, although those matters are of less moment than in a general hospital, yet they are deserving consideration; but a more important element in the discussion is the character of the cases to be received, and the conditions and length of residence accorded. Now, St. Luke's Hospital was especially instituted for the medical treatment of recent cases of insanity, and the residence of patients was by rule limited to one year. Is this hospital, therefore, so utterly unsuited for this its special purpose, and under the conditions of residence fixed, as the Lunacy Commissioners declare? We are convinced it is not. We are equally convinced that the chances of recovery for recently attacked lunatics are, from its small size and its medical organisation, better than in any of our large county asylums. Indeed, its statistics, even after all due allowance made for the selection of cases required by its rules, demonstrate its value as a hospital for the cure of insanity, unequalled by any asylum in the kingdom. The disadvantages it labours under in situation, in space for out-door occupations and exercise, and its heavy, gloomy architecture—which, by the way, is most conspicuous on the outside—are more than counterbalanced by the facilities for individual treatment and supervision, and by the acquired skill and experience of its staff in dealing with recent mental disorder. These disadvantages, moreover, are not so great, with regard to the treatment of recent cases, as those under which such cases are placed in large county establishments, where they constitute the exceptional beings, and cannot reckon on the same prolonged individual care and treatment; where attendants are less versed in their necessary management; where a rigid routine is demanded for the due ordering of the large population, and the new patient ranks merely as an addition, and probably an objectionable and ill-regulated one, to the many occupants of the ward,—in all probability to a large extent chronic patients and undesirable companions and examples.

The Commissioners evidently never realise the special features of St. Luke's. They examine and criticise it with a model

county asylum before them for comparison. Their own personal experience was gained in large county establishments, and they seem unable to avoid applying that experience when they sit in judgment on another sort of institution, to which it is not fairly applicable. The requirements of a population of chronic lunatics are not in all points identical with those of one of recent cases, and *vice versâ*. The latter are examples of physical disease amenable to medical conjoined with moral treatment; the former instances of organic cerebral change beyond remedial agents, and needing only safe custody and opportunities of employment and entertainment, to wile away the remainder of their days as happily and usefully as their condition allows.

We might dilate upon and illustrate largely the position we have taken up, but the length of this paper prohibits it. Yet before leaving this topic of St. Luke's Hospital we must express our regret at the decline in the usefulness and in the financial position of that charity. In past years its wards were well filled, and some 200 cases of recent insanity were annually admitted to its benefits, of which nearly 70 per cent. were discharged recovered. It gave gratuitous relief to a section of the poorer middle class unable to pay for accommodation in private institutions, and it rescued them from pauperisation. We fear it was an erroneous policy that led to its partial transformation into an establishment for receiving paying patients. At least it was a departure from its fundamental purpose as a charity, and would appear to have brought no profit.

In April, 1869, the Commissioners reported the presence of only 129 patients, of whom no more than 56 were recent cases, the rest being incurables. It is hard to explain the causes of this falling off in numbers when all other asylums are overcrowded. Some unpopularity must have gathered around it, and some degree of this might be attributed probably to the repeated representations of it as an unfit place for treating the insane. Unfit, and very unfit, it most certainly is for the residence of chronic cases; and if the institution be obliged to find accommodation for such cases it should devise a scheme for providing it in the shape of an auxiliary establishment in the country. Such a scheme, moreover, might be worked most beneficially in the interests of the recent cases and of the whole institution, and do away with some objections that fairly belong to its site and construction

J. T. ARLIDGE.

XII.—Medical Thermometry.¹

THERE is no better example of the truth that the success of a discovery depends upon the time of its appearance than that of the value of thermometry in disease. In very early times it was known that those suffering from disease often presented marked changes of temperature; but this knowledge was not formulated nor its full meaning perceived until the present day. That the fluctuations of temperature in disease are now accurately traced, measured, and noted, depends partly upon the far greater scientific ability and the more thorough training of modern clinical observers; but it depends also upon the corresponding advance of the collateral sciences, and derives both its interest and its means of growth from its participation in this general advance, and from the significance which would have been wanting to it in times when its phenomena stood alone.

There was no reason why the thermometrical measurements of Sanctorius, of Boerhaave and Van Swieten, of De Haen, of John Hunter, and of Currie, should not have been carried on by other observers, and established in doctrines. Everything necessary for the prosecution of such researches was at hand, and many very interesting observations were indeed made by the authors I have quoted, and by some others; but the period was not ripe for it; no rational idea of the essential meaning of the changes could be entertained until the more general laws of heat were known, and until it was established that the human body is at one in its movements with the forces without it, and until the laws of the generation of heat in the body and its correlation with the "vital" and all other forces were discovered and confirmed. It is this solidarity between the vast onward movement of modern medicine and that of the other sciences which makes modern medicine so inspiring a study, and which is now drawing into our profession the ablest men of the time.

That elevation of the profession of medicine which is seen in the improvement of practitioners at large, in the higher estimation of the calling by the older universities, in the breaking up of the antiquated routine of the medical academies, in the closer alliance of medicine with statecraft, and in many other such signs as these, is due to the fact that it is becoming a worthier calling, and is surely gathering into its ranks the highest intelligence.

The sciences accessory to medicine are pursued for their own

¹ *Das Verhalten der Eigenwärme in Krankheiten.* Von Dr. C. A. WUNDERLICH Professor der Klinik an der Universität Leipzig, &c. &c. Leipzig. 1868.

sakes, and as parts of the whole scheme of knowledge, so that medicine has the services of physicists, chemists, and biologists, each of whom, labouring directly for the furtherance of his own science, indirectly labours for us.

The student of medicine must now educate himself in the methods of the exact sciences, of optics, hydraulics, electricity, heat, of chemistry also, and of experimental physiology, so that he brings to the mystery of life that habit of mind which will lead him, with Huxley, to regard it as an "orderly mystery," and that method which will require him to weigh, measure, and co-ordinate its phenomena, which method alone can produce results of lasting value. At the same time we are still at liberty, as Dr. Anstie says, to use "a frank empiricism" in those districts which exact investigation has not as yet reached. Meantime, the researches of Fraser, Crum Brown, Liebreich, W. B. Richardson, Broadbent, and others, are leading up by ways of great promise to the most precious kind of therapeutical knowledge.

In all these respects medical thermometry is of the deepest significance and importance. It gives us, perhaps, the most intimate knowledge we can have of the variations of functional balance. It is the best means of recognising a sound constitution; it is the most subtle indication of disorder; it is the most accurate measure of conditions affecting the body, whether in health or disease; and it is the most trustworthy test of the operation of drugs.

As yet thermometrical observation is a recent industry, and must be said to be, if not in its infancy, at least in its youth; many interesting essays have been written upon it, and many physicians are now at work upon it, but as yet only one treatise has been published which deals adequately with the whole subject. This treatise (that of Wunderlich) displays, however, so much perseverance and thoroughness, such admirable caution and insight, and such wide and minute learning, that it may be said, not only to establish this branch of investigation for the first time upon a deep and lasting basis, but also to build up a very great part of the edifice, and to point out with clearness the directions in which future labour must be applied.

I need not say, therefore, how much I am indebted to Dr. Wunderlich for the materials of this review; indeed, had it not been for his treatise, my article could not have been written, or could only have dealt with a small part of the subject. It would be tedious to the reader were I to acknowledge my debt to this author in every page, and I prefer to content myself at the outset with this general expression of it. At the same time I may add that I have consulted at first hand almost every original

essay on medical thermometry, whether by home or continental writers, and that thermometry has formed a part of almost all my own clinical investigations ever since I read Mr. Simon's article in 1860, and the pages on the same subject in the first edition of Aitken's 'Medicine.'

Before passing on to the subject of practical experience in thermometry, I propose to make a few remarks upon its historical aspects. This I do in no curious spirit, nor even to serve the larger ends of history itself, for which this is not the occasion, but because a reference to some points in the history of thermometry will make the meaning of the present practice of it more distinct; for in this, as in all other scientific studies, the mental attitude of the observer is an integral part of the process of his observations. That heat was a pathognomonic symptom of fever was, of course, known from the earliest times of rational medicine; it was commented upon by Hippocrates, by Galen, and by the Arabians, and it has long been a matter of popular knowledge; but the relation in which this increment of heat stands to the bodily processes has only been suspected in very recent times, and cannot as yet be set down as known. It is rather surprising in times when cold, as well as heat, was regarded as a positive existence, and when so much importance was attached to the various combinations of the two qualities and their degrees of opposition, that no attempt was made to measure either. Nothing could show more clearly the fecundity of the inductive method and the sterility of metaphysical doctrines. It is also remarkable that, after the discovery of a thermometer, in obedience to the needs of physical observers, and, indeed, after the application of positive methods to the investigation of disease, the measurement of temperature in febrile diseases was so long overlooked. But this delay accords with our observation of progress in other departments of knowledge.

Temperature variations are less appreciable, and depend upon more complex and less known laws; their study was, therefore, postponed for the study of the simpler mechanical phenomena, of the changes in the circulation, for example, which were more easy to determine, and which so obtained an undue prominence. Though then the changes of temperature were recognised, and accurate instruments at hand to measure them, yet they remained unrecorded until a beam of light was thrown upon the whole meaning of temperature in the body by the brilliant discoveries of Liebig, Mayer, Helmholtz, and Joule. These great men revolutionised our conceptions of heat and its relations, and made an epoch, not only in the simpler sciences, but also in physiology.

It is but just, however, to remember the observations of older physicians. Sanctorius, in 1638, had constructed and employed a thermometer for use in disease, and, what is more interesting still, had brought heat fluctuations into parallel with the fluctuations of bodily weight. For a hundred years little more was heard of the thermometer. Boerhaave makes the next allusion to it, but his attention was chiefly given to the mechanical phenomena of the circulation; for he says, "*Velocior cordis contractio, cum aucta resistentia ad capillaria, febris omnis acutæ ideam absolvit.*"¹ But his scholar Van Swieten, while still saying that "*pulsus aucta velocitas*" is the pathognomonic sign of fever, yet has the acumen to point out also that the heat of fever should be recorded by thermometers, because the sensibility of the physician's hand is quite untrustworthy as a medium of observation. He recommends the mercurial thermometers made by Fahrenheit, as being "*pulcherrima et portabilia quidem*" as well as "*accuratissima.*" He used to put the instrument in the mouth or the axilla, but either his instruments or his methods were not "*accuratissima,*" for he gives us some very loose results.² Very different were the observations of De Haen. Before the publication of Wanderlich's treatise, I had lighted upon De Haen's thermometrical studies while consulting him for another purpose. I then made many extracts from his remarks upon temperature, and very interesting they are. They are scattered throughout the volumes of his '*Ratio Medendi,*' and deal with many important points. For instance, he had observed the temperatures of healthy men, and had discovered the increase of temperature in the aged. In diseases he had evidently made a careful study of temperature, as he himself says "*Non autem deciesve, sed pluries ipsissima experimenta iterata sunt et semper idem docuerunt.*" He had discovered the rise in temperature during rigor, the morning and evening fluctuations, the want of parallel in many cases between the temperature and the pulse, the frequent contrast between actual temperatures and the patient's perceptions, the settling of temperature as a sign of convalescence, the thermometrical indications of the actions of drugs, and many other points. It is strange that in the days of few books these observations of De Haen had so little influence upon others.³ In England several observers of the eighteenth century had studied animal temperature, and I have taken some interest in collect-

¹ '*Aph.,*' 581.

² Van Swieten, '*Comment.,*' Lugd., 1745, tom. ii, p. 26.

³ See, particularly, De Haen, '*Rat. Med.,*' ii, 10; iii, 3; iv, 6; vii, 5; x, 1, 2; xii, 2, &c. &c.

ing and comparing their opinions. Wunderlich refers to some of these writers, and more especially to the experiments of Blagden in the 'Philosophical Transactions.' These experiments, though communicated by Blagden, were really due to Fordyce, who was, however, joined in his researches by Blagden, and also by Banks and Solander. By exposing themselves to high temperatures in heated chambers these observers established the independence of animal heat, and proved, as they say, that the animal body has the "power of destroying heat." This is clearly due, they add, "to a principle of life;" for what "chemists or mechanical physicians" have been able to imitate such a process? While referring to these articles I came across a remark in a letter by Governor Ellis,¹ where, in describing the great heat of Georgia, he says, "a thermometer hanging at the end of my nose would often stand at 105°, while in close contact with my body I could never get it above 98°."

I have found some similar observations, also, by the Abbé Chappe d'Außerroche, who describes the Russian baths in his 'Voyage en Sibérie' (tom. i, p. 51), and in the 'Memoires de l'Académie des Sciences' for 1764 are some experiments by Tillet on the bearing of heat by animals. Dr. Martin, in the third volume of the 'Edinburgh Medical Essays,' takes great pains to show that animal heat is caused by the attrition of the blood upon the sides of the containing vessels. He is a strong disciple of the mechanical school, all of whom looked for the causes of animal heat in the velocities of the blood, and he was opposed by John Hunter, who in a quarto volume called 'Observations on the Animal Œconomy,' published in 1786, says, "It is, then, most probable that it (the heat) arises from some other principle; a principle so connected with life that it can and does act independently of circulation, sensation and volition, and is that power which preserves and regulates the internal machine." He notes that the temperature of man is 1.5° (!) lower during sleep,² and he gives a plate of a thermometer with an ingenious sliding scale which he used for experimental purposes. One or two writers hint that in putrid fevers the heat is due to the putrefaction; but others reply that, as heat is known to cause putrefaction, the heat in fevers is rather the cause than the effect. Cullen adds little to the controversy; he speculates on the differences of heat in animals being due to differences in their vital principles, and altogether he is not happy on this question.

¹ Vide 'Phil. Tr.,' vol. 1, p. 755.

² During sleep the relation between production and dissipation of heat remains unchanged.

The chemists treated the inquiry more ably than the physiologists. The great Lavoisier, who is quoted by Wunderlich, was working more fruitfully, if not more methodically, in conjunction with Laplace, and by them animal heat, in 1780, was referred to the combination of oxygen with hydrogen and carbon in respiration. I must claim attention to the fact that Dr. Black also declared that animal heat was generated in the lungs by respiration. Against these views was urged the rise of temperature after death, established by De Haen, and also the independent heat of the foetus.¹

The "absurd attempts of men who never extended their views beyond the laboratory, and, imagining their influence unbounded, attempted to explain all operations of the animal machine by ferment or mixture," were resented by the physiologists, and the discussion was somewhat barren until there appeared at the close of the century the work of Currie, a work destined to a far higher reputation than it has yet obtained, and entitled 'Medical Reports on the Effect of Water, Cold and Warm, as a Remedy in Fever and other Diseases.' It passed through many editions, but seems, nevertheless, to have exercised but little real influence upon contemporary opinion. This remarkable man based the whole of his observations upon thermometry; he made careful records of the temperature in his case histories, and he proved, in the same way, the relative power of various modes of treatment. He says, moreover, "A careful attention to the changes of animal heat, and to the state of those functions on which it depends and by which it is regulated, though more requisite in febrile diseases, perhaps, than in others, is, however, of importance throughout the whole circle of diseases." In our own century the subject gained more way, and Brodie gave a new turn to the question by referring to the nervous system as the source of animal heat.² Brodie was followed by several home and foreign writers among the physiologists; and Chossat, in his well-known essay on animal heat, referred more particularly to the sympathetic system as the source of heat. The chemists manfully withstood this tendency of the physiologists to disinherit them, and the echoes of the controversy are still resounding in our ears. From this time numerous experiments were made and carefully recorded. Among the chief were those of Breschet and Becquerel, made with the thermo-electric apparatus, and tending to establish a comparison between the local temperatures of different parts of the body.³

¹ It is interesting to find that by Black, Haller, and many other observers, the relation of red blood-corpuscles to temperature was recognised.

² *Vide* 'Phil. Trans.,' 1811, p. 36, and *ibid.*, 1812, p. 378.

Cf. 'Annales d. Sci. Nat.,' 2 sér., 'Zool.,' vols. iii, iv, and ix, 1835.

A good summary of the state of knowledge at the time will be found in Todd's 'Cyclopædia.'¹ We shall not be surprised to find Bouillaud and Andral² in the first rank of those clinical observers who seized upon thermometry as a true method of investigating disease, and in the books of both we find a number of accurate records. In Germany, which has been the chief centre of medical thermometry, much was done at this time.³ Fortunately Dr. John Davy stepped in to save the honour of English men of science, and published that series of laborious records so well known, collected in his 'Physiological Researches.' I cannot help speaking with much affection of this book, which came into my hands on its publication in 1863, and which was one of my first guides in thermometry; it cannot be called an unerring guide, but, in addition to the spirit of accuracy and faithful labour which animates it, we have in it also a collection of observations of permanent value. Dr. Davy's researches carry us forward beyond the time of the great discoveries of Liebig, Mayer, Helmholtz, and Joule, whose brilliant revelations, appearing almost at once, carried the dawn into full day. Liebig, as a chemist, following out the work of Black and Lavoisier, proposed those conceptions of animal combustion which, modified as they have been by his followers, must, nevertheless, be to his immortal honour. At the same moment Mayer, Helmholtz, Faraday, Joule, and Grove, by their grand idea of the circulation of one force in many forms throughout all nature, organic and inorganic, and of the mutual conversion without loss of these forms one into another, placed us upon a pinnacle of observation.

Carpenter industriously applied these principles to the animal body, and completed the doctrine that the activities of histological elements are translated into equivalents of heat, of movement, of chemical products, and of intellectual energy. In 1850, then, the scheme of work which lay before us as physiologists and physicians was completed, and Bärensprung and Traube, initiating the era of active medical thermometry, stepped into the field at the same moment. Since that time medical thermometry has found many able investigators in Germany. Wun-

¹ Vide Art. by Edwards.

² Cf. Bouillaud, 'Clin. Méd. de la Charité,' tom. i, 293-4. It was very interesting to see these two veterans re-open the subject of thermometry four months ago. Vide 'Comptes Rendus de l'Acad. des Sciences,' Dec. 6, 1869.

³ Cf. *e.g.* Gierse, prize dissert. on the 'Cause of Organic Heat in Inflamed Parts,' 1842, and Zimmermann, who made a large number of valuable observations, which are scattered throughout many periodicals; references to his papers are given by Wunderlich, p. 44.

derlich took up Traube's work in 1851, and in his volume published in 1868 he may be said to sum up the results of almost twenty years' researches. If in England we have little to show which will compare in extent and accuracy with the German observations, this is due in a great degree to the very different conditions of our medical professoriate, which chiefly consists of physicians occupied in practice. At the same time we, too, are able to point with pride to such researches as those of Dr. Parkes, who, by his investigations into the tissue changes of febrile disorders, has contributed largely to the extent and accuracy of modern doctrines. I shall endeavour, so far as is possible in a short essay, to review these doctrines as we now have them. We believe, first of all, that the balance of heat in the human body is very nearly constant for all conditions, and that the narrower the limits of variation the more sound and stable the individual constitution. Secondly, we believe that in the numerous disorders which are attended with variations of temperature above or below the normal, such variations stand in a very intimate relation to the variations of the whole bodily processes. Thirdly, we believe that in the thermometer we have an instrument of sensitive and exact observation, one which determines the patient's state without the bias of subjective influences, and also unbiassed by the prepossessions of the observer. The thermometer, then, gives us, definitely and simply, and at any moment, an accurate key to those general variations in waste which otherwise could only be detected by prolonged and laborious search, or which might as frequently escape notice altogether.

That the normal standard of human temperature should be so invariable for all conditions was not beforehand to be expected; but it is the establishment of this standard of measurement which makes medical thermometry possible. If there was not a certain known and constant regulation of waste, if its degree depended without any regulation upon all and any accidents which might affect it, then thermometrical observations would be altogether illusory, as having no foundation. It is equally valuable in practice to know that while the temperature of the healthy body is so uniform, the temperature of the sick body is so fluctuating; also that the organism which in health resists powerful influences brought to bear upon it so long as these are not morbid, yet in disease becomes very sensitive to influences of far less power. It is evident that in these propositions lies the whole secret of the method, and to them, therefore, we must give our early attention. Before passing on, however, to the comparison of records taken in the healthy body, let me say a few words upon the means and the methods neces-

sary to ensure the ease and the accuracy of recording. I need not say that degrees of temperature cannot be measured by the touch of the hand, and can least be appreciated in those cases where measurements are most important. Again, certain conditions of radiation or of superficial congestion may give a sensation of excessive heat to the touch when no such excess is present.

On the other hand, too sensitive an instrument is to be avoided, except in special researches, as the indications are too delicate for ordinary purposes. For certain ends I have myself used the thermo-electric instrument, but I have found that the mercurial thermometer is the best for common use, and is, of course, far more portable. Indeed, in choosing even a mercurial thermometer, it is not well to have too sensitive an instrument. The first clinical thermometers made in this country were those of Casella, under the direction of Dr. Aitken, and for hospital purposes they cannot be improved. They are, however, too cumbrous for general practice, and for this reason the thermometer long remained a stranger to the busy practitioner.

I hope it is no undue presumption on my part to please myself with the notion of having had a share in introducing the thermometer into general practice. A few years ago, when convinced of the vast improvement, both direct and indirect, which would follow the general use of thermometry, I began to think how the labour of carrying Aitken's instruments from place to place might be lessened. I applied to Casella, but he had no pocket thermometers, and seemed indisposed to make any. I therefore set to work with Messrs. Harvey and Reynolds to manufacture one; and it was not so easy as it may seem to devise an accurate thermometer of a new form, and with a safe and portable case.

The instrument was made in numbers by Messrs. Harvey and Reynolds, and the sale, at first slow, has during the last three years increased rapidly. I am glad to see the same instrument is now being made and sold by Hawksley and others, so that I have reason to suppose the thermometer is now in the hands of most medical men as a daily companion. It is a matter for much regret that these instruments are all made on the Fahrenheit scale. It is an inconvenient scale in itself, and it has the additional disadvantage of making English and foreign observers mutually unintelligible. There is really no excuse for its retention, as no human being supposes it to have any merit of its own. The new scale is very easy to learn, and the change would not unsettle the mind of the nation, as the use of thermometers is confined to a minority of competent persons. I urged Mr. Reynolds to make his thermometers on the centi-

grade scale, which has the additional advantage of being much more distinct in a short instrument than the Fahrenheit, but it stopped the sale promptly. He has very recently made an instrument having both scales upon it, but no one buys it. Do let me urge my readers to have the nerve to make this change at once, for it must come sooner or later, and meanwhile authors are spoiling their records for posterity.

Every thermometer should be tested, not only when purchased, but also every six or twelve months subsequently, as the molecular changes in the glass, caused by alternate expansions and contractions, tend to a certain degree of permanency. Any such permanent change, with the date of testing, should be noted on the case of the instrument. The instrument which I commonly use was correct when purchased, but as I have been so fortunate as to keep it for about three years, it now registers two tenths too high. This change took place during the first ten months of its use; and it has since that time been stationary. It is easy to allow for this kind of incorrectness at each observation; but if the bore should change in calibre, so as to make the spaces of elevation unequal for equal increments of heat, then the best thing to do is to destroy the instrument at once.

Most busy practitioners will need more than one thermometer, as it is often necessary to leave instruments with the nurses of the sick; in this case they should be numbered or otherwise distinguished. A coloured band round the case is a good distinction. As to sensibility, it is, generally speaking, true that the sensibility depends upon the size of the reservoir, and all directions as to the time of imbedding the instrument must be made with due regard to the size of the reservoir. The old round bulb is decidedly the best kind of reservoir for measurements in the axilla, but we cannot always have the best possible, and in this instance a long oval is practically better, as being quicker in visible expansion. We cannot wait for a slow rise; and, moreover, we want an instrument which, if occasion require, we may insert into the rectum or vagina. On the other hand, a certain sluggishness is desirable, for too sprightly an instrument may gain time at the expense of steadiness.

The length of six inches in the stem is useful, in order that the instrument may be more easily inserted, watched, and removed, and six inches is within the length of a stethoscope;¹ but a three-inch thermometer may be as accurate as a six-inch. I constantly use a three-inch thermometer of Messrs. Harvey and

¹ When I first designed pocket-thermometers I made them to fit within stethoscopes, but I did not like the result myself, nor did others. Dr. C. Fox has lately proposed this arrangement again.

Reynolds', made for me more than a year ago, and it is very accurate. I use it for observations upon myself, in this way: I wrap a band of wash leather around the upper scale, which I do not need, and thus the instrument may be imbedded under the tongue, and the projecting end held by the teeth without fear of displacement. With this instrument in my mouth I have gone through many exercises—such as climbing, diving, &c.—which would be impossible with any other.

For some experiments upon the healthy subject, however, a non-registering instrument is required; no instrument is better for this than Aitken's curved thermometer, which can be watched in position with or without a little mirror.

Next, as to the place of application, I nearly always make use of the mouth in observing myself, but in others this plan is liable to various misleading influences. For patients I always use the axilla. A discussion arose upon the point at the last meeting of the British Medical Association, and doubt was thrown upon the value of records in the axilla.¹ I had not then made any comparative experiments between records in the axilla and in the rectum, but I have since made many, and find the returns from the axilla so uniform as to inspire confidence, and so valuable in practice as to give the information I want; still, if the temperatures in the rectum do not run parallel to those in the axilla, a grave difficulty exists. For my part, I have found the temperatures of the rectum quite parallel with those carefully taken in the axilla, except in cases of collapse, when the temperature of the outer body falls, and that of the inner body rises. Here again, also, we must look to what is practicable. If patients allow of single rectal observations, which is doubtful, they will certainly rebel against their frequent repetition; and this is as true of the coarser as of the more sensitive natures, for in the former class of patients my assistants and myself have by such examinations excited comments, the narration of which would not tend to edification.

Again, in restless patients there is the very serious risk of breaking the instrument and leaving pieces of glass two inches within the anus; in others it excites reflex emptying of the bowel, or, short of that, rectal spasms, which tend to raise the local temperature.² Finally, the bulb may often be imbedded in a faecal mass, which has a temperature of its own. Anal examinations must, therefore, be rare, and must be confined to a few cases of extreme emaciation and to cases of col-

¹ Many recent observers have contrasted the records of the mouth and the axilla as between internal and external parts. The axilla, well closed, ought to be as much an internal part as the mouth, or nearly so.

² This cause of error is pointed out by Billroth.

lapse. In children I generally put the instrument between the thigh and the body, but sometimes in the anus. Children are often alarmed by axillary observations, and in these cases we do better to make them out of sight.

In making observations in the axilla, besides the precautions, which are too well known to need repetition, the axilla should be wiped dry from sweat, and then closed a few minutes before the insertion. If this be done the instrument may be removed a little sooner. If the patient be somnolent, violent, or thin, the thermometer must be held in place. There are two other technical points which need some remarks. These are, firstly, the duration; secondly, the frequency of the insertion of the instrument. In discussing both these points it must be always borne in mind that we have to prove the value of the thermometer in general practice, and to commend it to busy men; we must, therefore, be very careful not to frighten them with elaborate and tedious rules. Fortunately the frequent and protracted insertions, so much dwelt upon by some writers, are, under ordinary circumstances, quite unnecessary. In making exhaustive researches for standard purposes, no time and no care can be too much; but for the ordinary ends of practice two observations a day, each of four minutes' duration, is sufficient, even in bad cases. If the thermometer is an enemy to slovenly practice we may be glad, and surely in a bad case there ought to be time enough for the medical man to wipe the axilla when he enters the room, to close it for a minute or two, and then to insert the thermometer, gently warmed, for four minutes. Six minutes in all are thus required, and during this time the pulse and respirations may be counted and other inquiries made. If the instrument remain for fifteen minutes more it may gain one or two tenths, or it may lose as much; but unless we are dealing with very high degrees indeed, such as 106° and upwards, the difference of two tenths is as unimportant as the difference of a few beats in the pulse. Certainly no opinion can be based on differences so slight. A slight fall is quite as probable as a rise, for differences like these are easily set up in the sick by such trifling causes as food, drinks, evacuations, exertion, and the like, and mean little. Indeed, a distinctly engraved thermometer, marked to half degrees only, is quite sufficient for ordinary purposes. So it is again with repetitions.

In special cases, however, at special times, in insidious or capricious diseases, or about times of crisis, it may be needful to take measurements every two hours, or even every half hour, for that matter; but such instances are quite exceptional, and when they occur an intelligent attendant is easily taught to

make trustworthy records. Far from annoying either patients or their friends, thermometry generally commends itself quickly to their good sense, and is well carried out. The observations of nurses, indeed, are on the average much better than those of students, as they have nothing to distract them. In hospitals it is well to use Aitken's curved non-registering instruments, and to have them *in situ* at the time of the house-physician's visits. He then quickly transfers the number to a chart, and places with it the number of the pulse and of the respirations. In the great majority of cases two observations a day are quite enough, and these should be taken between 7 a.m. and 9 a.m. for remissions, and between 4 p.m. and 6 p.m. for exacerbations. It is all important that, whenever made, the hour of observation should be noted. Of the value of single observations to the consulting practitioner I shall speak more at length hereafter.

T. CLIFFORD ALLBUTT.

(To be continued.)

XIII.—Hospital Efficiency.¹

THE consideration of hospitals, in reference to their utility and their salubrity, as compared with the so-called cottage-hospital system, and with treatment at the homes of the humbler classes, cannot be better prefaced than by the following quotation from the 'Sixth Report of the Medical Officer of the Privy Council,' 1863:

"English rural hospitals have acquired, on false grounds, a reputation for comparative healthiness; by their regulations, their practice, or their position, they receive habitually a far less serious class of cases than is admitted into the hospitals of London and other large towns. This difference in the quality of the practice is much greater in respect of medicine than of surgery, but is considerable, even as regards surgery. The result is marked lowness of death-

¹ 1. *Sixth Report of the Medical Officer of the Privy Council*, 1863.

2. *Hospitalism and Zymotic Diseases*. By EVORY KENNEDY, M.D., &c. London, 1869.

3. *Circular Report by the Surgeon-General of the Army Medical Service, in the United States, on the Organisation of Military Hospitals* (quoted in the 'Lancet').

4. *Outline of Observations on Hospital Gangrene as it manifested itself in the Confederate Armies during the American Civil War, 1861—65*. By JOSEPH JONES, M.D. New Orleans, 1869, &c.

5. *Reports of the Dublin Obstetrical Society* ('Dublin Quarterly Journal of Medical Science,' 1869 and 1870.

6. *Edinburgh Medical Journal*, December, 1869, and January, 1870 (several papers), and previous reviews on *Hospital Construction*, on *Scandinavian Medical Literature*, &c., in the 'British and Foreign Medico-Chirurgical Review,' 1866, 1869, 1870.

rates, even (in many cases) in the presence of a high degree of hospital insalubrity."

Sir Astley Cooper it was, if our recollection serves us aright, who dwelt upon the circumstance that many classes of our city populations encounter very great risk, even from the most trivial wounds,¹ owing to the tendency to run to unfavorable issue. Brewers' men, butchers, vintners, and many others possess constitutions ill suited to bear up against operations or injuries.

Sir Benjamin Brodie wrote as follows :

"It is not to be expected that the artisans in crowded cities, living in close habitations, and to a great extent indulging in intemperate and thriftless habits, can enjoy the robust health and the physical powers of a rural population. There needs no other proof of this fact than the difference in the actual mortality of the two classes."

We shall have occasion to advert to this fact as recorded in the observations of other most competent and impartial writers in the course of this review. It is reasoning, therefore, upon utterly false data to compare the results of operations performed upon rural and city patients together, on account of the generally totally disproportionate powers of endurance and reparation possessed by the two classes. As stated by Dr. Bristowe and Mr. Holmes, the nature of the cases, both medical and surgical, varies greatly in town and country ; in the former for the most part vastly exceeding in gravity those met with in country hospital and in country private practice. It is for these reasons that the statement of Mr. Callender, which we quote subsequently, possesses especial interest. Many of our readers will, doubtless, be aware of the facility with which wounds heal in the natives of many eastern countries, whose simple modes of living favour such a result.

The American Government set an example worthy of consideration by other nations in the system of hospitals adopted during the late war. Some of our contemporaries have wisely drawn public attention to the results achieved during that memorable struggle, which so far surpass what has been accomplished in European armies, that we will briefly notice them. Partial amputation of the foot was followed by a mortality of 9 per cent. in the American army, while in the French during the Crimean war it was 48 per cent. For amputation of the leg the result was, in the American army, 26, in the English army 65, and in that of France 71. In amputation of the thigh the relative mortality was 64, 65, and 91 ; also that of arm amputation, 21, 25, and 55. As our contemporary, the '*Lancet*,'

¹ Eminent Continental writers have dwelt upon the same fact.—REVIEWER.

observes, it is not with any intention of under-estimating the surgical skill of our Transatlantic brethren that we attribute much of this eminently satisfactory result to the "unusually favorable physical conditions surrounding the wounded, such as an efficient hospital organisation and a superior system of transport."

The following observations of the Surgeon-General of the Army Medical Service in the United States, on the organisation of military hospitals, we give in his own words (Circular, p. 152):

"Never before in the history of the world was so vast a system of hospitals brought into existence in so short a time. Never before were such establishments, in time of war, so little crowded or so liberally supplied. They differed, too, from the hospitals of other nations, in being under the command of medical officers. Instead of placing at the head of establishments intended for the treatment of disease and wounds officers of the line, who, whatever their other accomplishments might be, could not be expected to understand the requirements of medical science, and who, with the best intentions in the world, might seriously embarrass the action of the surgeons, as was sadly the case in the Crimean war, and has since been the case in the English hospitals, our Government, with a wiser discretion, made the surgeon the commandant of the hospital, and thus while holding him responsible for the results of its management, put it into his power to do much to make those results favorable. The medical staff can point with pride to the consequences of this liberal course. Never before in the history of the world *has the mortality in military hospitals been so small*, and never have such establishments *so completely escaped from diseases generated within their walls.*"

Professor Lister¹ claims to have obtained most reliable results by his system of antiseptic treatment in hospital cases. He asserts that he almost entirely abolished pyæmia, erysipelas, and hospital gangrene from his wards, and that he has reduced the mortality after amputation from 1 death in every $2\frac{1}{5}$ cases, the ratio before he commenced the antiseptic treatment, to 1 death in every $6\frac{2}{3}$ cases. He does not confine himself to carbolic acid, but uses chloride of zinc as more suitable in some special cases.²

Turning to the subject of Maternities, Dr. E. Kennedy thus writes:—"Having failed, through my fellow-governors, to

¹ 'British Medical Journal,' 1868, and 'Lancet,' 1867 and 1870.

² Mr. Morton, of Glasgow, from comparative trials of carbolic acid, oil, common putty and oil, dry lint, and water dressing, used as dressing in surgical cases has not been at all satisfied of the superiority of carbolic acid dressing. He gives a series of cases, confirming his statement, attended by himself in the Glasgow Royal Infirmary, 'Lancet,' February 5, 1870, p. 188.

modify our great institutions to meet the urgency of the case, it was determined [by him, Dr. Kennedy] to challenge a full discussion of the question on the part of the medical profession." He then goes on to observe that "this was done by reading a paper before the society" (Obstetrical of Ireland), and he now leaves the "public to judge whether truth rests with himself or with the seventeen physicians" who combated his statements.

The concluding paragraph of the author's dedication to Mr. Gladstone claims notice; it is this:—"Should I be fortunate enough to secure your approbation for this paper, the justice and propriety of its objects will be established, despite the defects of its execution. . . . The tenor and purport of my observations shall be directed principally to prevention, whilst I hope some hints may be thrown out in the course of our investigation that may also assist us in treatment." It will be seen hereafter that Dr. Kennedy's views on treatment differ from those held by the profession generally.

To substantiate his conclusions he asks the profession to agree, "for argument sake, that zymotic diseases result, in nine out of ten cases, from preventible causes. It follows, then, that by the prevention of these causes, the mortality may be reduced one fourth, minus one tenth." Many will doubtless think, with those who combated this statement, that we have no right to assume the preventibility of what many now include under the name of zymotic disease in the proportion stated by Dr. Kennedy. Indeed, there is no little inconsistency in his advocating the sweeping away of all lying-in hospitals, while admitting that, in those constantly ventilated, puerperal fever does not occur in a virulent form; and that its appearance in properly regulated hospitals, is not remarkable, as it is also "met in the hovels of the poor, and even amongst the dwellings of the wealthy."

The cottages¹ proposed by Dr. Kennedy, with two or at most three patients each, spread over the sloping ground surrounded with lofty mansions, in broken lines on each side of Rutland Square, with higher ground to the north, would, in the opinion of an "eminent architect, have their ventilation [on which Dr. Kennedy with judgment so much relies], and even chimney draught, quite retarded."

The late Dr. Collins after detailing the prevalence of puerperal fever in the early years of his Mastership of the Lying-in Hospital,

¹ February 12, 1870.—From a communication we have just received we learn that Dr. E. Kennedy has abandoned the cottage hospital plan, and now recommends instead, communication from ward to ward by galleries external to the building.

and the success of the steps taken by him to lessen it is thus quoted by Dr. Kennedy:—

“The facts here detailed are strongly calculated not only to lead us to suspect, but even prove that this fever derived its origin from some local cause, and not from anything noxious in the atmosphere. To this I should assent, had we not proof equally well authenticated, of its prevalence and fatality in the houses of the affluent, as already stated.”

Dr. Kennedy comments thus upon Dr. Collins’ statement:—“Collins’ paragraph, above quoted, contains the gist of the puerperal fever difficulty in a nutshell. Its local cause approaches more nearly to a *constant* quantity in the wards of a crowded lying-in-hospital; whereas it is only an *occasional* quantity in the houses of the affluent,” &c.

For ourselves we cannot see how the remarks about ‘*constant quantity*’ and ‘*occasional quantity*’ can be deduced from Dr. Collins’ statement, which, like its author, is practical and straightforward, and signifies that the opinion of that able master was, that when that which we may call the atmospheric influence, or, as Dr. Collins well expresses it, “anything noxious in the atmosphere” exists, all are alike (whether they be affluent and dwelling in their own homes, or poor and sojourning in hospitals) affected thereby.

Dr. Kennedy’s treatise is eminently discursive, but we cannot see the connection between the alleged corroboration which he states he has found in Dr. Collins’ statement and his own, as above collated, and the following views propounded by him:

“A malady thus zymotic in its type and origin, produced by a poison emanating from parturient women”—a statement combated by Dr. Stokes—“more active in proportion to the concentration of their excretion or exhalations, and consequently in proportion to their number cohabiting in a given number of feet of atmospheric space; but not requiring more than one parturient female to generate it; when the poison she herself has generated may, as in the case of blood-poisoning, be re-absorbed into her own system, and self-contaminated then as certainly strike her down as if a crowded ill-ventilated lying-in ward was the generating medium.”

Dr. Kennedy here argues that one (unhealthy, we presume he means) parturient woman may be idiopathically attacked with puerperal fever as readily in her own dwelling as in “a crowded ill-ventilated lying-in ward.” This no one denies, but Dr. Kennedy would lead his hearers (though we understand from himself he entirely failed to do), or his readers to believe that he regarded the wards of lying-in-hospitals at the present day as being crowded and ill-ventilated. He then quotes gaol fever (which, however, he states, is now little met with), erysipelas in surgical hospitals, cholera in our camps and overcrowded human gatherings, tuberculosis among Paris workmen and apes, pyæmia, hospital gangrene, glanders among horses, &c.

While speaking of Dr. Kennedy's idea of ill ventilation, we remember to have heard his suggestion (upon a visit to the famous Dublin Lying-in Hospital) for what he conceived an improvement in its ventilation, viz., that the original plan should be departed from, and that the doors of opposite wards should be altered to prevent, as he alleged, the spread of contagion from one ward to another. The effect of such a step would be to prevent the free dilution of the atmosphere of the wards, as is now ensured by the very arrangement which he condemns. And, indeed, Dr. Johnston, the present Master, points out that the construction of the hospital is well adapted to secure free ventilation. Dr. Kennedy, however, discovers it to be faulty, and, on principles new to us, asserts :

“A generally commingling atmosphere is thus secured through doors, staircases, and perforated openings, so constructed that isolation is totally impossible, as it is impossible to open a door, or to go in or out of any room in the building, without diffusing the tainted atmosphere, if it exist, throughout the whole structure.”

He forgets the “windows, the sashes of which are constantly, more or less, open,” the doors “always open,” as mentioned in the subjoined extract (p. 447) from Dr. Johnston's ‘Report,’ and the excellent chimney draughts and unimpeachably clean wards, together with the total absence of bed curtains,—all ensuring an entire freedom from anything in the slightest degree resembling “the tainted atmosphere” suggested, with, however, the proviso “*if it exist.*”

Doubts of the exactness of Dr. Kennedy's statements, induced by certain inconsistencies therein, might be more than confirmed by Dr. Johnston's report for the year ending 5th November, 1869, from which we extract the following :

“Ten wards were set apart for confinements—I say ‘were’ as one of them (No. 11) has since October last been added to the wards for diseases of women—viz. eight in the large or principal building, and two in the auxiliary building, which stands separate from the other at a distance of seventy-two feet to the west, and in advance of it by some twenty-six feet; its back wall being so far more south than the front wall of the hospital.¹

“There are four large wards on each floor of the main building; those on the upper storey are numbered 1, 2, 3, and 4 respectively; Nos. 1 and 2 being opposite to one another at the west end, while Nos. 3 and 4 occupy a similar position at the east end; and each opens on a wide corridor running east and west 133 feet, with large windows at either end, the sashes of which are constantly more or

¹ See Dr. Phelan's letter in ‘Medical Press and Circular,’ for January 20, 1869, where he says, “a large house adjoins it on the west side.”

less open. The ward at the western end, on the south side of the corridor, is separated from that on the east by a space of at least thirty-five feet occupied by the chapel; those on the northern side are separated from each other by a room fully twenty feet in width, having a large window, which, with the door, is always open; and the entrance doors to each of the wards on the northern side are about sixty feet from each other; those on the southern side are the same.

“The wards Nos. 5, 6, 7, and 8, on the lower corridor, are immediately under those we have been describing, and are similarly circumstanced, with the exception of the wards on the northern side, which are separated by a wide staircase with a large Venetian window, which is seldom closed. On the south side, as on the upper storey, the chapel occupies the intervening space.

“The two wards in the auxiliary building consist of two rooms each, one with a southern, the other with a northern aspect, and are connected by a door which is always open. The wards on the first floor are numbered 12, those on the second floor No. 11. These last have, since the beginning of October last, been appropriated to patients labouring under uterine complaints. Each of the large wards contains seven beds, one of which is occupied by the nurse or a midwife pupil; but we seldom have more than four of the others occupied by labour patients, unless when many are coming in, and then five may be taken into the ward; so that, as each room has a cubic space of thirty-four by twenty-four feet, and thirteen in height, each patient has, on an average, 2000 cubic feet of air; and when I mention that thorough ventilation is kept up and extreme cleanliness observed, not only as regards the ward itself, but also in strict attention to the person of the patient, the atmosphere is quite free from any impure air; and, in fact, I may safely say that such a thing as ‘a loaded state of the atmosphere’ does not exist.”

In this report also Dr. Johnston repeats the remark made by him in course of the discussion on zymotic disease at the Obstetrical Society of Ireland, “that it could not be admitted that zymotic disease (puerperal fever) prevails *endemically* in hospitals *generally*,” although we have no hesitation in allowing that it may appear occasionally in those which may be ill-ventilated or overcrowded, and that it was injustice to put forward the dogma that “where puerperal fever is epidemic, it shows itself generally in our great maternity hospitals in the first instance.” Such—

“Is not the case. On the contrary, how frequently have we known of it prevailing in other quarters, amongst patients confined in their own homes, and even among the affluent, when the hospital was perfectly exempt from sickness. Nay, how much rather should we look for these diseases in the localities from whence those seeking admission emanate—in the narrow, filthy, unswept streets; the courts and alleys, in too many instances, reeking with the pestilential effluvia of the half putrid offal and ordure, which, by imperfect sewerage, or no

sewerage at all, allows the noxious gases escaping therefrom to pervade the overcrowded, small, unwashed, ill-ventilated apartments; their bedding, if possessed of such a luxury, saturated with filth and dirt; the unfortunate occupants frequently in a weak, emaciated state, from want, penury, starvation, and disease—in fact, everything tending to produce a poison which cannot be otherwise than injurious, and most likely to engender the malady we have all so much reason to fear.”

We would here pause to remind our readers of the fact observed in some of the Continental hospitals, namely, that when patients were admitted some days before delivery, the death rate was notably reduced; a fact we can readily understand, from our own experience in the matter, inasmuch as the cleanliness, comfortable bedding, and suitable food of a well-regulated hospital go a long way in improving the health of patients, and rendering them far less liable to the diseases and accidents of childbirth.

Dr. Johnston thus continues:

“Contrast those miserable dwellings with the large, lofty, and airy wards of our institution, where perfect thorough ventilation is observed; where the patients are not crowded together, but ample cubic space of air is allowed for each; where the strictest cleanliness in every manner is maintained, and every attention paid to their wants and well-being by liberal diet, beef-tea, wine, &c.—is it not reasonable to come to the conclusion that, instead of the hospital being the habitat for zymotic disease, it should rather be attributed to the wretched abodes of those poor creatures who seek its shelter.”

Of 1,159 patients delivered in the hospital, 937 were cases of purely natural labour, 26 tedious, 47 preternatural—viz. 7 upper extremity, and 40 lower do.—81 were forceps, and 4 were craniotomy cases; 9 were delivered by version; in 1 the perineum, and in 1 the uterus had to be incised; 27 had twins, 11 were cases of accidental hemorrhage; 1 a case of placenta previa, 4 were convulsion cases; in 10 there was prolapse of the funis, 1 was a case of induced labour at the eighth month, and 1 a case of hydatids. Moreover, in 8 cases mania succeeded labour.

Twelve deaths from zymotic disease occurred within the year; the particulars of each case are given; in no instance did the disease spread to a second person. Six of these cases had been ill before and on admission, two were “victims of seduction,” one had been deserted by her husband, and “turned out” by her relatives when in bad health; another, also in bad health, was unmarried. In 2 cases labour was tedious, and chloroform and the forceps were resorted to; and in another case erysipelas appeared two hours and a half after delivery.

“Small hospitals¹ are now said to afford to patients greater chances of recovery than large hospitals. Cottages are said to afford still better chances than small hospitals.” Dr. Duncan does not admit the validity of the kind of statistical reasoning adduced against hospitals; and he confines his inquiry to “obstetrical data,” considering it universally admitted that “the data of maternity hospitals offer us excellent means of conducting this inquiry.” He first examines the data of one small hospital; secondly, those of a promiscuous collection of small hospitals; thirdly, those of some highly praised small hospitals.

The Royal Maternity Hospital of Edinburgh, “under the care of a body of distinguished physicians, with Sir James Simpson at their head,” who “accepts a special yearly vote of thanks for his part in the management of the institution and the treatment of the patients,” being a small institution with considerably less than 300 deliveries yearly, is judiciously cited as an instance of the much vaunted small hospital; yet we find the average mortality in it during the years 1844 to 1846 was 1 in 61, and in the year 1867 no less than 1 in 43: whereas—

“In the great Dublin hospital, the largest in Great Britain, with its more than a century of existence, and more than 190,000 cases, the mortality was 1 in 72. During the late Dr. Collins’ mastership, in an experience of above 16,000 cases, in seven years, the mortality was 1 in 100.”

Dr. Duncan observes—“This looks dark for small hospitals. One, that may be called Sir James Simpson’s, presents a picture which would have deterred most men from asserting that small hospitals are more salubrious than large ones; but all does not settle the question; it only shows that smallness is a quality of an hospital which offers no security for superior so-called salubrity. It shows this irrefragably. Other individual small hospitals might be cited to show the same thing.”

A promiscuous collection of data of small hospitals, gathered respectively by Dr. Churchill and by Le Fort, the enemy of hospitals, follows. The former author quotes from eleven sources, comprising 27,300 cases, and calculates 1 death in 67 in small hospitals. Drs. Collins, Johnston and Sinclair, in 28,528 cases, in the Dublin large hospital, had only a mortality of 1 in 123.

It is necessary to remark that the mortality of large hospitals, of 1 in 62, as calculated by Dr. Churchill, is based on statistics derived from such institutions, among others, as are to be found in Paris and St. Petersburg, having a mortality quite extraordinary, and far above anything known in this country.

¹ *The Alleged Salubrity of Small Hospitals.* By J. MATTHEWS DUNCAN, M.D. (*Medical Times and Gazette*, Nov. 13, 1869.)

Le Fort reports of institutions receiving annually under 100 lying-in women, a mortality of 1 in 12; and of larger hospitals, one varying from 1 in 23 to 1 in 37. Dr. Duncan writes—

“What, then, does this Le Fort method of calculating tell regarding small hospitals? It makes them to be the worst of all. The smallest have the most frightful mortality. Naturally Le Fort says, when describing the data which we have above given, the statistical researches are there, to prove that one must not seek the cause of the greater or less mortality of the lying-in, in the size of the establishments, and that all hospital hygiene cannot be reduced to questions of architecture. . . . It is easy to see, in running over the figures, that the relative mortality in the small and large maternity hospitals does not formally plead in favour of the small establishments.”

As Dr. Duncan observes, Le Fort's remarks require no further comment.

(To be continued.)

¹ ‘Des Maternités,’ p. 75.

Bibliographical Record.

Gray's Anatomy.¹

A LARGE proportion of our readers will be familiar with the contents of 'Gray's Anatomy,' and look back with thankfulness to the help its admirable engravings afforded them whilst still learners of anatomy. For this valuable and complete treatise has now been before the world some twelve years. That its worth is well recognised, no other proof need be furnished than that, although so extensive a volume, and so full and minute in detail, it has reached a fifth edition. And we apprehend that it will maintain its ground as the most comprehensive and lucid English work on anatomy ever published, for several generations of students yet to come.

But we must extend our estimate of its value, by remarking, that it is a work necessary also to the surgeon throughout life; for the attrition of other labours, and of the cares of the world in after-life, is a cause of anatomical denudation to the best of us, and it needs the counteracting agency of repeated fresh deposits derived from some ever-available storehouses of anatomy to prevent denudation proceeding to obliteration.

From the well-known scope and character of the previous editions of this treatise, nothing remains to be said touching the contents of the present issue, except so far as novel matter is introduced. Some such matter presents itself, however, for observation. The editor tells us in the preface, that an introductory chapter on general anatomy has been appended. Information heretofore scattered through the volume has been collected, and an outline of microscopic and general anatomy produced. This course, we are further told, has been taken "in deference to the opinions of persons very competent to judge, and who believe that some such addition is necessary to the completion of 'Gray's Anatomy.'" At the same time, "it is not intended to supersede or trench upon the treatises on physiology, nor to go minutely into the more recondite and more dubious parts of microscopic research. Nor, again, is it intended to give any account in this work of vital phenomena."

We must confess ourselves dubious of the expediency of this newly

¹ *Anatomy, Descriptive and Surgical.* By HENRY GRAY, F.R.S., &c. Fifth Edition. With an Introduction on General Anatomy and Development. By T. HOLMES, M.A., &c. London, 1869. Pp. 768.

introduced chapter. It adds above 100 pages to a volume previously inconveniently large. It is a departure from the scope of the work as represented by its title, as a treatise on anatomy descriptive and surgical. It presents, as indeed is honestly confessed, only a résumé of the teachings of the accepted works on microscopic anatomy and physiology. It attempts an undesirable divorce between minute structure and the functions associated with it, and so deprives the student of the light that structure and function are calculated to throw the one upon the other; and lastly, as is confessed in the preface, it presents nothing more than an imperfect outline, having the more recondite results of research left out.

Why should not the student go at once to the complete works on physiology and microscopic anatomy, and fully acquire the lesson he must learn? What is the advantage to him of such an introductory chapter as that here presented, when his physiological text-book contains all the information it vouchsafes, and more to boot? Is it not almost as much out of place and uncalled-for as a chapter on physiology in a treatise on organic chemistry?

Holden's Osteology.¹

It would be enough to announce the appearance of a new edition of so elaborate and highly esteemed a book as Holden on the bones, inasmuch as its character and contents are so well known and so fully calculated to maintain for it the first position among books on the like subject. Such a course would, moreover, suggest itself on the present occasion, since this new edition is but a reprint of the text of the last one, with only "a word or a sentence here and there thrown in for the sake of clearer description or fuller explanation," and the redrawing of the illustrations, the original plates being worn out. But we cannot refrain remarking on the fact of a fourth edition being called for of so considerable and costly a treatise on the bones only, as of good omen with respect to the thoroughness of the work done by students of the present day. For as a book specially addressed to students, and calculated to meet their requirements, we must conclude that the three previous editions have almost entirely been purchased by them.

The supplementary chapter, on "the unity of type in the construction of the vertebrate skeleton," can serve no other useful purpose than that of directing the student's attention to a general

¹ *Human Osteology; comprising a Description of the Bones, with Delineations of the Attachments of the Muscles, the General and Microscopic Structure of Bone, and its Development.* To which is added a brief notice of the Unity of Type in the Construction of the Vertebral Skeleton. By LUTHER HOLDEN, F.R.C.S., &c. Fourth Edition. Illustrated by numerous Drawings on Stone, and Woodcuts. London, 1869. Pp. 282.

fact; for, in the present state of comparative osteology and amid so wide diversities of opinion as to typical forms and nomenclature, it would be but a waste of time and of labour for the learner in anatomy to "get up" the hypotheses and difficult nomenclature of Professor Owen, to which Mr. Holden introduces them in the chapter in question, and concerning which the student might draw the erroneous inference, that they are facts and doctrines accepted by the scientific world without dispute.

It might be objected to the chapter on the Larynx, that it is an uncalled for interpolation in a book on the bones. It, moreover, only furnishes a description of the framework,—the cartilages and ligaments, and consequently leaves it to the student to complete his knowledge of the organ in anatomical and physiological works—where too, he will encounter a repetition of the account of the particular structures Mr. Holden has already detailed. Exception may also be taken to the description of the internal ear given in this volume, as incompletely conveying the present extent of information relative to that organ, and we would particularly remark on the absence of reference to the valuable researches of Dr. Jago, published in this review.

Heath's *Practical Anatomy*.¹

THE appearance of a new edition of this excellent manual of dissection will be welcomed by anatomical students. Its descriptions and directions are conveyed in clear and concise words, and are, moreover, made distinct to the eyes by the very numerous well-executed engravings scattered throughout the work.

Besides undergoing a general revision at the hands of the author this second edition contains some new matter, particularly in relation to the anatomy of the brain, a subject which has been diligently worked at of late years by several anatomists with the view of making its description and the nomenclature of its parts more definite and precise.

Mr. Heath adds that he has likewise introduced a series of sectional views of the limbs and trunk, in order to enable the student to test his knowledge after dissection.

With these few remarks on the changes in this second edition, we commend the treatise to all students of practical anatomy.

Kirkes' *Physiology*.²

THIS handbook won from the first the good-will of students for

¹ *Practical Anatomy: a Manual of Dissections*. By CHRISTOPHER HEATH, F.R.C.S., &c. Second Edition. London, 1869. Pp. 596.

² *Handbook of Physiology*. By W. S. KIRKES, M.D. Seventh Edition. Edited by W. MORRANT BAKER, F.R.C.S., &c. With Two Hundred and Forty-one Illustrations. London, 1869. Pp. 838.

the character and extent of the information it conveyed, and from the lucid manner in which that information was set forward. Verily it has had its reward, in having arrived at a seventh edition, and secured to itself a position that may render it almost impervious to hostile criticism. To maintain that position requires the diligent attention of the editor, who takes upon himself the preparation of a new edition, to the plan of its original esteemed author, to make it truly represent the accepted physiological doctrines of the day.

Mr. Morratt Baker professes to have kept this object in view, and to have recorded all important and well founded recent observations. This may be so generally, but on glancing through the work there have appeared to us some omissions, and the retention of some physiological speculations that have been supplanted by more recent research. For instance, Liebig's views of food and of its connection with animal temperature require revision, and the details presented respecting temperature in health and in disease are not up to the present standard of information as represented in the treatise of Wunderlich, and elsewhere in well known essays. So again, on the question of the distribution and termination of nerves, the observations of Dr. Beale are not given; and, in regard to the minute structure of the nervous centres, we miss reference to the important researches of Lockhart Clark. Farther, in discussing the physiology of special nerve centres we notice the absence of a record of much recent inquiry, and whilst favoured with a sketch of Gall and Spurzheim's opinions, we discover no account of the attempts to localise speech in the anterior hemispheres. The grand question of protoplasm now agitating the scientific world, is kept out of sight.

The editor may point us to the preface to the sixth edition, and contend that he is only carrying out the principle there propounded to himself in revising the original work, viz. to abstain, "as rigidly as possible, from inserting all contradictory statements, raw theories, and descriptions of imperfectly or ill-observed facts." Nevertheless, we believe he would find few to coincide with him in esteeming the facts and hypotheses we have alluded to as raw theories and imperfect observations; and we regret to have to state the opinion that this last edition does not bring up the information contained in this text book to the standard of the day, even after making all due allowance for it as an elementary treatise addressed to learners. The list of references to books and essays quoted in the substance of the treatise confirms this opinion, that the latest researches have been passed by; for few indeed are the references to authorities who have written within the space of the last ten years.

Journal of Anatomy and Physiology.¹

THIS most valuable scientific journal continues to present original contributions on anatomy and physiology of the highest interest and character. The number now before us equals any of its predecessors in the importance of its contents. Besides articles on comparative anatomy there are several demanding the attention of the student of human physiology; among which may be noted:—Dr. Blake, on the action of the compounds of chlorine, bromine, and iodine, when introduced directly into the blood; Dr. Broadbent, on the so-called selective absorption by the lacteals and lymphatics; Dr. Hensley, note on the arrangement of the muscular fibres of the ventricles of the heart; Dr. Chiene, note of a supernumerary lobe to the right lung; Dr. Brunton, on the chemical composition of the nuclei of blood-corpuscles; Dr. Carter on the distal communication of the blood-vessels with the lymphatics, and a diaplasmatic system of vessels; Mr. Ray Lankester on the spectroscopic examination of certain animal substances; Professor Turner on supernumerary cervical ribs; Dr. Ransome, observations upon the movements of the chest; and Professor Turner, note of a case of displacement of the submaxillary glands.

The important paper by Dr. Carter is illustrated by a coloured plate of the diaplasmatic vessels described in it; and those by Dr. Ransome and Mr. Ray Lankester by diagrams.

Besides original articles the journal contains some brief notices of books, and reports on the progress of anatomy and of physiology, by Professor Turner and Drs. Fraser, Rutherford, and Gamgee; and also “notices of recent Dutch and Scandinavian contributions to anatomical and physiological science” by Dr. W. D. Moore. These reports on anatomy and physiology are very complete, and bring under the reader’s notice almost every recent fact in those sciences to be gathered from British and Foreign scientific journals.

Odling's Lectures on Carbon.²

THESE six lectures were given to a juvenile auditory in the Royal Institution during the Christmas holidays of 1868-9. They were reported verbatim and published in the ‘Chemical News.’ We are indebted to Mr. Crookes for his very carefully executed republication of them in a separate form.

¹ *Journal of Anatomy and Physiology*. Conducted by G. M. HUMPHRY, M.D., F.R.S., and W. TURNER, M.B., &c. Second Series. No. V. November, 1869.

² *A Course of Six Lectures on the Chemical Changes of Carbon*. By W. ODLING, M.B., F.R.S. Edited by W. CROOKES, F.R.S. London, 1869. Pp. xii, 162.

Dr. Odling's mode of handling his subject is at once interesting and appropriate. He takes common things, objects with which every child is in a sense familiar, and then compares them, contrasts them, and submits them to experiment. A bit of marble, a piece of scorched paper, a stick of charcoal, lend themselves readily to his method of teaching. From the obvious and known characters of these bodies Dr. Odling proceeds to the obscure and intricate, so that not only are the fragments of knowledge already possessed by his hearers enlarged, but these are gradually welded together so as to make an harmonious whole.

In those schools where systematic lectures on the elements of chemistry cannot be given, it would be quite a feasible task to arrange for the delivery of these six lectures on the chemical changes of carbon. The experiments are easily performed, and do not involve the use of costly apparatus, while at the same time the subjects treated of are of the very highest importance.

From marble our author takes us to the change which it suffers when heated or acted on by an acid. The study of carbonic gas or carbon dioxide, its properties and decomposition, next engages our attention; then the carbon and the oxygen which it contains. The characteristics and uses of the various forms of carbon, diamond, graphite, charcoal, coke, and lamp-black, are discussed at length, as well as a number of other related topics, such as the migrations and functions of carbon and carbon dioxide in the three kingdoms of nature.

Odling's Outlines of Chemistry.¹

THE value of this book to teachers of chemistry will be found very considerable. Of course it is not, and is not intended to be, a readable book. It is a full series of notes; an abstract, in fact, of the lectures given by its author at St. Bartholomew's Hospital. A student by reference to these Outlines will be able to anticipate or to recall the subject of any particular chemical discourse. Every word not absolutely necessary to the sense is omitted; and the result of this method of writing, combined with Dr. Odling's erudition and wide range of research, has been the production of a most compact yet comprehensive manual of chemistry.

In order to give a fairer and more adequate notion of Dr. Odling's treatment of his subject than could be done by mere description, we will quote a few paragraphs from the couple of pages (pp. 263—265) which he devotes to the metal *mercury*.

“Purification of foul mercury, by its exposure to air under oil of vitriol, or by its agitation with dilute nitric acid containing some

¹ *Outlines of Chemistry, or Brief Notes of Chemical Facts.* By WILLIAM ODLING, M.B., F.R.S. London, 1870. Pp. xiii, 468.

nitrate of mercury in solution, or most efficiently by its redistillation, preferably in current of superheated steam. Mercury reduced to the state of minute non-adherent globules by agitation with certain solutions, as of mercurous nitrate, and converted into non-lustrous powder or paste by being rubbed up with chalk, grease, &c. Its precipitation, in yet finer state of division from solution of corrosive sublimate by stannous chloride or other reducing agent. Solution real or apparent, of different metals in mercury. Separation of semisolid amalgams by forcibly filtering off the excess of mercury through leather. Production of definite compounds, Ag_2Hg_2 , CuHg , &c., by subjecting the crude amalgams to powerful pressure."

We may just say in conclusion, that the nomenclature mainly employed by Dr. Odling, is that which is daily gaining more complete acceptance in this country, namely, that which has been adopted by Roscoe in his excellent and popular 'Elementary Chemistry.' But although Dr. Odling usually speaks of silver nitrate and mercurous chloride, he allows himself the latitude (almost unavoidable in extempore speaking) of occasionally using such terms as nitrate of silver, or the trivial expressions—lunar caustic, and calomel.

Gill's Chemistry for Schools.¹

THIS book is at once descriptive and practical. After each account of an element or of a compound, directions for performing illustrative experiments are given, and these are followed by questions as to the phenomena observed and the inferences to be drawn from them. Mr. Gill's little volume is well put together, accurate, and readable. It has, moreover, no exact counterpart amongst the numerous chemical manuals lately issued from the press. It is more full of manipulation and experiment than Roscoe's 'Elementary Chemistry;' it is more descriptive, and begins more completely *ab initio* than the 'Laboratory Teaching' of Bloxam. In schools where it is proposed to use but one text-book of chemistry for the whole of the science, Mr. Gill's volume will be likely to prove suitable. It would, however, be better fitted for this purpose had a chapter on some of the chief carbon compounds been inserted.

Medical Retrospects.²

THESE two useful volumes of abstracts from the current medical

¹ *Chemistry for Schools: an Introduction to the Practical Study of Chemistry.* By C. H. GILL. London, 1869. Pp. xv, 315.

² *The Retrospect of Medicine.* Edited by W. BRAITHWAITE, M.D., and JAMES BRAITHWAITE, M.D. Vol. ix. July—December, 1869. London, 1870. Pp. 408.
The Half-yearly Abstract of the Medical Sciences. Edited by W. DOMETT STONE, M.D., &c. Vol. i. July—December, 1869. London, 1870. Pp. 372.

publications of the day deservedly hold their ground, each finding in its own group of admirers a sufficient *clientèle* to encourage the editors in the arduous work of selecting whatever has the claims of novelty or of apparent utility in the ever-widening area of contemporary medical literature. Indeed, the scope of these two half-yearly abstracts so far differs, that the subscriber to the one might advantageously also take in the other. For whilst the older of the two, viz., that edited by the Braithwaites, is entirely occupied in the analysis of British journals, the other, best known as Ranking's, takes a much wider field, and surveys the contents of foreign, as well as of British current literature.

Such retrospects particularly commend themselves to the busy practitioner, who cannot attempt to examine for himself the contents of the multitude of medical journals that now solicit his attention, and who has a praiseworthy desire to keep himself *au courant* with the pathology, and especially with the therapeutics, of the day.

Liverpool Medical and Surgical Reports.¹

WHILST the hospital reports published by some of our metropolitan institutions contain a large amount of valuable and eminently practical information from the pens of men deservedly held in high esteem for their professional attainments, there can be no doubt that many of our provincial hospitals and infirmaries have within themselves all the necessary elements for contributing their quota towards the general stock of medical and surgical knowledge. It is, however, unfortunately the case, we think, that either from diffidence or apathy (but certainly not from any want of ability) the officers of these charities seem but too frequently averse to publishing their experience in some such connected manner as that adopted by the London hospitals, although were they to follow the example set them they would assuredly enlighten at times their metropolitan confrères, and add materially, as well as generally, to the edification of the profession by a record of cases possessing more than an ordinary amount of interest.

Looking at the rapid increase in the population, and the general development manifested in some (indeed, we might say *all*) of our immense centres of commerce, it is not difficult to understand that both the range and variety of diseases, as well as the number of accidents brought under the notice of the medical and surgical staffs attached to the charities in such places, can be but very little, if any, less than those to be met with at most of the kindred institutions situated in our modern Babylon.

¹ *The Liverpool Medical and Surgical Reports.* Vol. iii. October, 1869. Edited by P. M. BRAIDWOOD, M.D., and REGINALD HARRISON, F.R.C.S., Assistant-surgeon to the Royal Infirmary, Pp. 140. London and Liverpool.

The pre-eminence of Liverpool as a seaport, with its seething and, in part, ever-changing population, must necessarily supply no small scope for practice to those who hold appointments at its hospitals and infirmaries; and although it appears that hitherto no one particular hospital staff has felt itself justified in publishing a record of its own doings, it is very satisfactory to find that a combination of talent has produced a highly creditable series of medical and surgical reports, the third volume of which we have now before us.

The present number consists of fifteen communications by the following contributors:—1. On Chorea, General and Local. Dr. Inman. 2. Tracheotomy in the Treatment of Croup and Diphtheria. Dr. Braidwood. 3. A Case of Lumbar Colotomy. Mr. Hakes. 4. Cases of Tracheotomy. Dr. Wollaston. 5. Cases of Mollities Ossium. Dr. Turnbull. 6. On Rupture of the Uterus. Dr. Ewing Whittle. 7. On the Use of Acid Nitrate of Mercury. Dr. Lyster. 8. On Nodding Convulsions. Dr. Davidson. 9. The immediate Treatment of Stricture of the Urethra. Mr. Harrison. 10. Note on Poisonous Dyes. Mr. Brown. 11. Carbolic Acid in Surgery. Mr. Hamilton. 12. On some peculiar Results of Injury to Digital Nerves. 13. Notes on the Operative Treatment of Inflammation of the Eyes. Mr. Walker. 14. On Sea-Sickness. Dr. De Zouche. 15. Clinical Observations on Submaxillary Cellulitis. Mr. Bickersteth. And, by way of an appendix, we suppose, the volume concludes with an abstract of the proceedings of the Liverpool Medical Institution, Session, 1868-9, and of the proceedings of the Liverpool Northern Medical Society, 1868.

The subjects included in the above list are, without any exception, possessed of considerable interest, and the method of their treatment by their several writers has just that practical character which cannot fail to make the reading pleasant as well as profitable.

Bigelow on the Hip-joint.¹

THE author's object in writing the present volume is to show the superiority of the method of reducing recent dislocations of the hip by manipulation with flexion, over the old-fashioned way with the pulleys. The introduction contains a carefully drawn up abstract of the propositions that the work is intended to prove, which will be found of great assistance.

The author, without denying that muscles exert both an active and a passive force, assumes that they play but a subordinate part in hindering reduction, which he considers due to the resistance of what he calls the Y ligament. Our text-books of anatomy describe a

¹ *The Mechanism of Dislocation and Fracture of the Hip, with the Reduction of the Dislocations by the Flexion Method.* By HENRY T. BIGELOW. Philadelphia. Pp. 150. Illustrated.

capsular ligament to the hip-joint, strengthened in front by a strong band called the ilio-femoral ligament. Dr. Bigelow describes this ligament as arising from the anterior inferior spinous process of the ilium, passing downward to the front of the femur, to be inserted fan-shape into nearly the whole of the oblique spiral line which connects the two trochanters in front, being half an inch wide at its iliac origin, and but little less than two inches and a half wide at its insertion. This ligament bifurcates into two principal fasciculi, one being inserted into the upper extremity of the anterior intertrochanteric line, and the other into the lower part of the same line about half an inch in front of the small trochanter. The ligament thus resembles an inverted Y, which suggests a short and convenient name for it. These fasciculi in a well-developed subject form an unyielding suspensory band of nearly a quarter of an inch in thickness.

Well illustrated examples of the position and influence of this ligament in the different luxations of the hip are given, from which the author, in opposition to the authority of Sir Astley Cooper, argues that pulleys are useless, tending only to elongate or even partly detach the femoral insertion of this powerful ligament, with violence to the tissues, a great sacrifice of mechanical force and uncertainty of result; whereas, by the flexion method, the resistance of the Y ligament is annulled and reduction easily effected.

Although Dr. Bigelow does not claim to have individually introduced the flexion method, which he admits was known as far back as the time of Hippocrates, yet great credit is due to him for the very clear and lucid descriptions of the various fractures and dislocations of the hip which he has now laid before the profession. They are the work of one well acquainted with his subject, illustrated and proved by most carefully made dissections. At the end of the volume are some excellent examples of the different fractures of the neck of the femur, with good wood-engravings of specimens obtained from the dissecting room.

The work is full of instruction, and will amply repay for its perusal; nor must we omit the meed of praise due to the publisher for the superior style in which it has been produced.

Niemeyer's Practical Medicine.¹

THE students of German literature in this country, though steadily increasing, are still comparatively few in number, and, what is more

¹ *A Text-book of Practical Medicine, with particular reference to Physiology and Pathological Anatomy.* By FELIX VON NIEMEYER, Professor of Pathology and Therapeutics, Director of the Medical Clinic of the University of Tübingen, Translated from the Seventh German Edition, by special permission of the Author, by GEORGE H. HUMPHREYS, M.D., and CHARLES E. HACKLEY, M.D., &c. Two Vols. New York, 1869.

to be regretted, they have hitherto done but little to help those in whom the power of acquiring languages has been either less cultivated, or is less developed. In this respect both France and America are far before us, for scarcely any work of importance published in the "Fatherland" escapes the French translator, and, happily for many here, an almost equal industry animates our brethren across the Atlantic. We are justly proud of Chance's translation of Virchow's 'Cellular Pathology,' and we believe the impetus given by it to pathological research in this country has been very great; but it is, nevertheless, a sad reflection that the other great work of the Berlin teacher on the 'Pathology of Tumours' as yet knows no English dress. It is now nine years since its publication, and about three years since the first volume of the French translation appeared, and yet the great majority of readers here know it only from articles in this Review and occasional references in other journals. The same can be said of dozens of books, which might long ago have found hundreds of appreciative readers if professional activity had only devoted itself to translating, instead of reproducing in diluted forms, the results obtained in that great modern workshop of human thought.

To America we owe a debt of gratitude for the work already done, and the debt is increased in no small degree by the translation of Niemeyer now before us. The publication of these volumes brings within the reach of the busy practitioner, the student, and the teacher in our schools, one of the best text-books in any language. As the translators say, "Professor Niemeyer's volumes present a concise and well-digested epitome of the results of ten years of carefully rendered clinical observation by the most illustrious medical authorities of Europe," elaborated, we may add, by a most able teacher and an experienced physician. Like 'Watson's Practice of Physic' in these islands, Niemeyer is a classic among the text-books of the Continent. To translate such a work worthily is no easy task, and we congratulate Drs. Humphreys and Hackley on their success. The style throughout is clear and good, and the rendering, though often close, has little, if any, of that ruggedness which so often mars an otherwise good translation.

Here and there scattered through the 1500 closely-printed pages the German words have been given when no exactly equivalent expression could be found. In one instance of this kind "fettige usur" is translated by "fatty consumption," which, to our mind, scarcely expresses the idea intended to be conveyed so well as the expression "fatty usure," used in the translation of the cellular pathology. Quotations from Hippocrates look better, too, in Greek character than in modern type. Not the least valuable part of the book will be found in the therapeutical sections, and the numerous formulæ, which are given most clearly, will prove a boon to many a hard-

worked practitioner. In the French translation of this book original notes were appended by M. Cornil, and many of them were most useful to the reader not acquainted with some of the peculiarities of German pathology. Every day, it is true, lessens the number to whom such notes are necessary; but we nevertheless suggest that a similar addition in the next issue might, perhaps, make the English translation more popular.

We cannot omit the praise due to the publisher. The type is good and clear, the paper excellent. The publisher has, in a word, done his work in the same spirit as the translators, and the result is very creditable to both.

The Food Journal.¹

A "TEEMING PRESS" was a phrase invented by our forefathers many a long year since, for what then appeared a most prolific engine in distributing the products of seething brains among the mass of mankind. But the productiveness then was as nothing to that of the present day, and our respected progenitors would stand aghast at the flood of periodical literature now poured forth, calculated to deluge mankind, if mankind did not almost as rapidly advance in its receptive faculties and powers of endurance. Sanitary science is itself a modern development, and assuredly claims to be heard by its own organ as fairly as any other specialised branch of knowledge. Hitherto, however, success has not attended the efforts made from time to time in this country to establish a periodical specially devoted to its promotion and elucidation. Another attempt is now made, and we trust it will meet with the success it deserves. It makes food its leading subject, and should by so doing secure for its readers all who "eat to live" as intelligent and happy beings. If it opened its pages to matters connected with food only, it would have a very wide field of work and usefulness. In a thickly populated country like our own the supply of food is a great social question, and at the present time arouses the attention of all thinking people. Another, and certainly an equally important question in the matter of food, is the system of adulteration carried on with reference to almost every article of consumption; and it is one that cannot be too prominently and persistently kept before the public. It was, however, wise of the projectors of the journal to open its pages to the description of matters of social and sanitary economy at large. Papers on these subjects appear in the two numbers already published.

The proprietors must be congratulated on the list of contributors they have secured, for we see on it the names of many of the best known sanitary reformers of the present day. The journal is also

¹ *The Food Journal: a Review of Social and Sanitary Economy, and Monthly Record of Food and Public Health.* London. Nos. 1 and 2. 1870.

excellently got up in the matter of type and paper, whilst its very moderate price of sixpence a number ought to obtain for it a wide circulation, and particularly where the knowledge it affords is so much needed, namely, among the middle and the upper working classes.

Tanner's *Practice of Medicine*.¹

THE rapidity with which edition after edition of this work has appeared and disappeared is, on the whole, a true test of its merits. The fifth edition was, we believe, an exceedingly large one, yet the book was for some time out of print before the present one could be prepared. Dr. Tanner has chosen his title well; his work is essentially one on the *practice* of medicine in its widest sense, and it is in what relates to pure practice, as contradistinguished from the theory of medicine, that the book is strongest; for it has been the author's aim to collect, within the boards of these two volumes everything he could think of which would aid the practitioner in the discharge of his duties. But it is not to men engaged in the active discharge of the duties of their profession alone to whom the book is welcome. With the student, preparing himself to enter upon these duties, the book has long been a favorite, chiefly, we believe, from the lucidity of its style and the character of its substance. Other books there are, more eloquent, and some more recondite, but none excel Dr. Tanner's work in those important features. All that it is necessary to know is here, disposed in such a manner as to admit of the readiest reference, and of being most easily retained in the memory.

Our space will not admit of an extended review, which would be out of place with regard to a book practically established as a standard, but we shall try to point out some of its leading features. The work as it now stands is divided into sixteen parts; of these, the first and most extensive deals with general diseases; the second with fevers; the third with venereal diseases; the fourth with diseases of the nervous system; the fifth with diseases of the organs of respiration and circulation; the sixth, which concludes the first volume, with diseases of the thoracic walls. Volume ii begins with diseases of the alimentary canal; part 8 comprehends diseases of the liver; 9, those of the spleen and pancreas; 10, those of the abdominal walls; part 11 deals with diseases of the urinary organs; part 12, which is long, treats of the diseases of the female organs of generation; parts 13 and 14 contain diseases of the skin and its appendages; part 15 includes diseases of the blood-vessels as they come before the physician; the last part refers to diseases of the absorbent system. Next comes an excellent collection of formulæ, with hints as to diet, change

¹ *The Practice of Medicine*. By THOMAS HAWKES TANNER, M.D., F.L.S., Member of the Royal College of Physicians, &c. In Two Vols. Sixth Edition. Enlarged and thoroughly Revised. London. Pp. 656 and 645.

of climate, and the selection of mineral waters. Finally, we encounter one of the most striking and most useful features of the work, an admirable index.

It will, perhaps, be most convenient to confine our remarks to the first of these parts, viz. that relating to general diseases, as this portion will constitute a fair, perhaps even somewhat trying, test of the rest of the book. Dr. Tanner begins with some account of morbid conditions of the blood, and this leads him to give an account of the constitution of normal blood, which is, we think, out of place in such a practical work (these things are better left to the physiologist), anything like an abnormality being, however, carefully noted. Under the head of anæmia, Dr. Tanner substantially includes Grave's disease, although it is also discussed elsewhere. Some men would, we imagine, dispute the validity of this, but Dr. Tanner has practical results on his side. Patients undoubtedly do better on mild ferruginous preparations than on anything else. Among abnormal blood conditions the author includes gluc hæmia (why the unconverted Y in this word?) Although he treats diabetes mellitus elsewhere, he is inclined to accept Dr. Pavy's views as correct, in which respect we are not altogether disposed to coincide with him, the later German investigators, such as Kühne, being rather unfavorable to them in their totality. In the condition known as acholia, Dr. Tanner would seem to adopt Dr. Austin Flintje's solution, that it is the cholestearine which is at fault. That normally this substance is excreted by the liver, appearing in the fæces as seroline, but being retained in the blood it poisons the system in a manner similar to that seen in uræmia. This theory has been so far countenanced abroad that the author's essay on the subject has been considered in France worthy of a large share of one of their most considerable sums of prize money.

In the section devoted to pyæmia, which, although very satisfactory on the whole, we find that Dr. Tanner has not seen fit to separate two closely allied yet diverse conditions—septicæmia and pyæmia. The former depends on the imbibition by the system of some poisonous matter, similar in its nature to malaria; whereas the latter is consequent on some injury or suppuration. Perhaps as good an example of septicæmia as could be quoted is what is sometimes called dissecting-room fever, consequent on too close attention to practical anatomy. Hueter of Greifswald, in his treatise on the subject, has well pointed out the distinctions between septicæmia and pyæmia. On the whole, Dr. Tanner's article on this subject is very satisfactory. He has no belief in the great value of the hyposulphites in such cases. Food, quinine, good nursing, and plenty of fresh air, are the prime requisites in treatment, and without them everything else is useless. Thrombosis and embolism are well discussed, and an interesting case is described. It always seems to us that these two conditions ought to be, as far as possible, discussed apart; the essential difference of

certain of the symptoms of the two would thereby be better appreciated, even although the one frequently induces the other. We question whether Dr. Tanner's section on *Hæmatozoa*, the only one of any importance being the *Bilharzia* of Egypt and Natal, would not be better described in connection with the intermittent hæmaturia it produces.

Scurvy is next brought on the *tapis*. In the treatment, and still more in the prevention of scurvy, food, not physic, is the rule; and although Dr. Tanner does point out the absolute necessity of dieting, we hardly think that he lays sufficient stress on the above proposition. Now-a-days scurvy should be unknown. In the treatment of the allied condition purpura, he recommends gallic and sulphuric acids; for our own part, we adhere to the iron salts, and, if necessary, give turpentine. The condition is invariably associated with debility.

As may be speedily learned from his article on inflammation, Dr. Tanner is a sworn foe to antiphlogistic remedies, especially bleeding; yet on this very subject there are doubts on the minds of many. A practice adopted by so many great men who have gone before us, and whose results are open for our inspection, cannot have been *altogether* bad. It is quite clear that a fair case has been made out for revising our practice, especially with regard to certain acute inflammations—pleurisy and the like.

The varieties of cancer recognised by Dr. Tanner are scirrhus or hard cancer, and medullary or soft cancer, with epithelial cancer, colloid or gelatiniform, melanoid, osteoid, hæmatoid, and villous cancer. Now, although this classification is, strictly speaking, not altogether accurate, still, from a clinical standpoint it indicates forms sufficiently recognisable. Two of the local applications for cancer which are most promising, viz. acetic acid and bromine, Dr. Tanner seems to have omitted.

So much has been done in the pathology of tuberculosis, even since Dr. Tanner wrote, that we could not expect to find here the latest results, still, a good, fair *résumé*, including all essential points, is laid before us. The most important matters which remain to be noted are the universal diffusion of lymphoid tissue wherever tubercle originates from pre-existing elements, and, which is a more doubtful question, the origin of tubercle from effete matters in the various organs of the body, such as cavernous glands, the result of scrofula or typhoid deposit. Dr. Tanner also devotes a short paragraph to scrofula without tubercle.

Two other morbid conditions, and we have done, viz. gout and rheumatism. In both of these we find that Dr. Tanner inclines to the chemical theory; in both instances we are sorry to disagree with him. We are not inclined to dispute the existence of uric acid in the blood in cases of gout, or of an acid in rheumatism (even the perspiration is *sometimes* alkaline); but we are strongly disposed to reject these as the efficient causes of disease. We have yet to learn how a few small doses

of colchicum could destroy or eliminate a vast accumulation of uric acid; it certainly relieves gout. The fact that alkalies do good is nothing. Alkalies, especially the salts of potass, are powerful in promoting the retrograde metamorphosis of albumen; and it is in this way, we take it, that they act beneficially both in gout and rheumatism. The lactic acid theory is not consonant with the phenomena of rheumatism, and we are not aware that it has ever been actually determined. Both forms of disease, we take it, are neuroses, accompanied by certain physical and chemical conditions, but not apparently depending on these. *En passant* we would remark that Dr. Tanner speaks of the good effects of Laville's gout liquid; these for the time being are undoubted, yet its votaries generally end by giving it up. Dr. Tanner seems also to have been misled by, what we fear we must call, the lying formula of its composition supplied with each bottle.

From what has been said our readers will have some notion of the book. It carries its own recommendation, and is its own best passport to general use. It has been the result of very great labour—labour well spent; and it appears in a form which is creditable to its publishers as it is pleasing to those who have to use the book. Both author and publisher may be pleased with the result of their labours.

Bang's Therapeutics.¹

In this "Manual of Therapeutics" we have a very condensed, but, at the same time, a remarkably clear and readable epitome of the experience of its veteran author, one who in a lengthened course of sixty years' practice, and during a period of office as Professor extending to upwards of half a century, has enjoyed the esteem and confidence of the profession, and earned the highest honours conferred on medicine by the State.

In his preface, Professor Bang informs us that, although in a university programme he appeared in 1822 as a zealous advocate of the most able of physicians—Nature,

"Many years had passed away before I learned fully to estimate all that she is capable of, before I perfectly comprehended the importance of not counteracting by ill-timed interference her efforts, her healing power. In the introduction to the first edition of this manual, in my 'General Therapeutics,' in many essays in the medical journals, in my popular medical works, I have since unhesitatingly endeavoured to give greater prominence to this view, to obtain for

¹ *Haandbog i Therapien*. Af Dr. Prof. OLUF LUNDT BANG, Conferentsraad, Storkors af Dannebrog, Commandeur af Vasa, Dannebrogsmann Kjöbenhavn. Den Gyldendalske Boghandel. MM. ANDEN OMARBEJDEDE UDGAVE. 1869, 8vo, pp. 463.

Manual of Therapeutics. By Professor D. OLUF LUND BANG, Privy Councillor, &c., &c. Second edition, revised. Copenhagen. 1869.

natural pathology, a more extensive recognition. This name appears to me to be most suitable for a doctrine, which starting from clinical observations of the proper working of the organism, seeks to remove what is injurious, to combat its results, to make good what is lost, to restore the equilibrium, and thus to preserve health and life. . . . There is no doubt that the physicians who study the curative methods of Nature, those who in her hail the most clear-sighted and most able colleague, and who are not ashamed to avail themselves of her guidance, must always be the most successful in practice."

A few pages further on he informs us that,—

"When in 1814 I had to lecture on general pathology, I followed, *mutatis mutandis*, none of the pathologists then living, but one of the best of those of the preceding century, Gaubius, whose doctrine, according to the knowledge of the day, was the soundest and least one-sided; I have never since rejected the diseases of the blood. . . . Where are the anomalies of disease sooner observed and discovered than in the blood, which, as the source of the solid parts, communicates to these its properties? Therefore, I have called the many diseases, which are by chemistry found or reasonably suspected in the blood, not diatheses, not constitutional diseases, not specialities, &c., but blood diseases—*dysæmiæ*—such a condition being, under all circumstances, the basis of the malassimilation of the whole organism in general, called *dyscrasia*."

The foregoing passages will probably suffice to put our readers in possession of the author's leading views; in a work intended to be "a manual for the physician, a synopsis for the lecture-room," and which is necessarily incapable of detailed analysis, little more remains for us than to state its general arrangement, and by an extract to illustrate the writer's very concise, clear, and accurate style.

"In this edition, as in the first, the diseases are omitted which more and more pass over into the hands of the specialists; those which occur in infants, in the external and internal senses, syphilitic diseases, some of those which are due to poison, and diseases of the female organs, so far as they require manual or instrumental help."

In the introduction Bang defines disease to be a subjective or objective anomaly arising in the organism, not rapidly disappearing without further consequences. He treats of the causes of disease under three classes; the first containing all that is necessary to life and health, as food, air, &c., but not corresponding in quantity or quality to the individual in his various circumstances; the second including everything absolutely injurious, external violence and all poisons; to the third may be referred medicines administered to the healthy, or not suitable to the disease. He speaks of the *vis medicatrix naturæ*, under the form of *hyperdynamia*, as an anomaly generally called a disease, but, in fact, most frequently at

the same time a remedy, exhibiting itself as *reaction*, removing what is injurious by excretion or by inflammation and its results; as *localization*, completely restoring health by crisis, or restoring the health of a part by metastasis, in the latter case making fresh efforts to subdue disease, or succumbing in its vain endeavours. A third method is *vicariation*, where a function is suppressed. We have also the *union* of dissolved continuity effected by plastic secretion, and the restoration of what is lost, brought about by increased metamorphosis of matter. Under the form of *adynamia* by producing syncope, a result of paralysis of the heart, it averts a bleeding vessel and saves life, or, as loathing, prevents the ingestion of food which the system may be incapable of utilising.

The author arranges all diseases under two heads, general and local. The former he divides into two great classes: morbi dynamici and morbi materiales. Morbi dynamici are subdivided into angio-pathia and neuropathia. The former may be hyperdynamic: active congestion, inflammation, fever; or adynamic: passive congestion (local), or lipothymia (universal). Neuropathia may in like manner be hyperdynamic—*Hyperdynamia sensitiva*, neuralgia, hypochondria, hysteria; or *H. motoria*, epilepsy, chorea, tetanus, &c.; or adynamic, paralysis, *Adynamia sensitiva*, anæsthesia; or *A. motoria*, acinesis. To the second great class belong material diseases, either chemical, suspected, or discoverable by analysis in the blood of the living organism; homopathia, subdivided into hyperæmia, anæmia, and dysæmia, or in the secretions; eccrinopathia (or organic), deviations in size, form, consistence, texture; trophopathia (or mechanical), which belong almost exclusively to surgery. Local diseases are divided, according to their seat, into encephalopathia, myelopathia, pneumopathia, cardiopathia, &c. Prof. Bang, in his introduction, treats, moreover, briefly of predisposition, diagnosis, prognosis, and treatment.

The author's description of Addison's disease may illustrate the style of the work, although the description is scarcely a fair one, as we have chosen the passage for its brevity:

“MORBUS ADDISONII, 104.—In the *bronze disease*—Melasma suprarenale—we observe light or dark-brown, bronze-coloured spots, of various sizes, on several parts of the skin—especially on the face (except the eyes), on the neck, abdomen, arms, or the whole body—rarely mixed with white—vitiligo—occasionally black, on the mucous membrane of the mouth, sometimes attended with dyspepsia and lumbar pains, always with diminution of condition and strength; consciousness unimpaired. After death, in most subjects, but not in all, disorganization of the suprarenal capsules is met with, which may, however, exist unaccompanied by this disease. It is easily distinguished from jaundice (87), lasts for years before its termination, which is always fatal. Treatment is in general useless, but cod-liver oil is said to have been tried with advantage” (p. 119).

Medical Directory.¹

The 'Medical Directory' has established for itself such a position and character as to render its possession almost a matter of necessity to every medical man, and to every public institution. It is much more valuable than any official register can be. It lets each medical man who will tell his own story of himself, and set forth his real or supposed merits, and so supplies those who refer to its pages with the best means obtainable of estimating the qualifications of those inscribed in its pages. Its value would be increased did every gentleman who receives the annual circular take pains to confirm the accuracy, or otherwise to correct the entries made of his qualifications, appointments and written contributions. As it is, a considerable proportion neglect this duty.

We have written this notice of the Directory not to proclaim its value, for this is sufficiently well appreciated, but to call the attention of its editors and proprietors to entries made by many gentlemen that needlessly swell its pages, and add to the cost of its production. Foremost among such are appointments as examiners and referees to insurance societies; as medical officers of friendly societies or clubs; as local secretaries of such institutions as the Medical Benevolent College, Epsom: for there is nothing in the tenure of such offices that partakes of a distinction. Every medical man acts, more or less frequently, as referee to insurance offices, and the holding of club appointments is a very ambiguous honour. Respecting the last sort of position noted, nothing can be said in apology for parading it, except it may be an individual can quote no other.

Again, as the Directory presents a complete account of the Poor Law Medical Service in each division of the kingdom, it seems an unnecessary thing to narrate the parochial appointments of each medical man who happens to hold such. Another entry of doubtful utility is that of member of the British Medical Association, seeing that every qualified medical man is eligible for the membership, and that it implies no superiority of one man over another, and that where one makes such a return, half-a-dozen others, equally entitled to it, refrain from so doing. It is also worth while remarking, that the Association annually publishes a list of its members. It is a difficult and a delicate task for the editors of the Directory to define the class of appointments and of medical and scientific distinctions of which they will accept and publish the returns, yet in the matter of those alluded to, such a course is

¹ *The Medical Directory for 1870, and General Medical Register: including the London and Provincial Medical Directory, the Medical Directory for Scotland, the Medical Directory for Ireland, &c.* London, 1870. Pp. 1845.

practicable because they are so generally distributed, and are universally regarded as so little worthy of being borne as badges of superiority by their possessors. There are, however, others not so easily disposed of, and the return or non-return of which must be left to the good taste and good sense of the individual possessing them. Such we regard are:—the appointment of examiner of recruits; of surgeon of an emigrant ship; of prosector at the College of Surgeons (a temporary post given to a candidate at the time of his examination), and also past appointments held during the term of education at hospital (such as clinical clerk and dresser), which most men who have exhibited ordinary industry have held. Membership of the Medico-Psychological Association, considered as a distinction, is much on a par with membership of the British Association, and is returned by only a tithe of those who can boast of it. Membership of local medical and of other societies for which the only material qualification is the annual subscription, and past membership of students' Societies, are also doubtful distinctions, and such as few men care for quoting in their returns. Indeed, did every practitioner whose name appears in the Directory follow the practice of some, and return all past and present appointments and distinctions, including those of doubtful dignity, the volume would be increased well nigh twofold, and that without addition to its value. Fortunately there is not such a wide-spread amount of vanity in the profession, and we hope to see, in future issues of this most important book of reference, a decreasing proportion.

Clinical Note Book.¹

THE publication of this little book is a laudable attempt to encourage case-taking. The busy medical man generally trusts to indistinct memory of certain cases that have made a more than common impression upon him, and to general deductions and impressions derived from the mass of cases that have come under his notice, the result being that indefinable quantity called experience; were, however, the salient points of cases written down, there would follow greater precision of observation, and a standing record of facts for the deduction of general conclusions, and consequently a measure of experience of a higher order. Dr. Fairbank's note-book will aid practitioners in attaining this higher standard of experience.

¹ *A Clinical Note Book for Hospital and Private Practice.* Edited by F. ROYSTON FAIRBANK, M.D., &c. London.

Original Communications.

I.—Report on a Case of Railway Injury. By WILLOUGHBY FRANCIS WADE, B.A., M.B., Physician to the General Hospital, Birmingham.

WHATEVER changes may be made in that branch of common law in which medical evidence is now available, it is certain that the necessity for skilled witnesses must remain, and that, upon the dicta of these, such cases must turn.

Should any of these changes diminish the number of such witnesses, or refer their opinion to be valued by medical assessors or the like, it will be more than ever important that the experts who remain should enunciate opinions based upon a comprehensive survey of the facts, and tempered by common sense, as well as logically coherent.

It is almost of necessity that the voices of ordinary practitioners, that is, of those who have had no special training or practice in forensic medicine, should still be heard. And it is much to be desired that medical evidence, whether ordinary or expert, should be freed from imperfections, which now often subject the individual to ridicule, and the body to which he belongs to contumelious criticism.

My experience of medico-legal cases has led me to form the opinion, that very many of the difficulties in which the members of our profession find themselves involved in a court of law, may be traced to one single root. I do not say all the difficulties, but my immediate object is, not to produce a handbook for medical witnesses, but to explain in a few words my reasons for publishing the following Report.

This special root, then, is the failure to make themselves masters of the case in all its bearings. It may be that the opinion held is correct, but it is not the less essential if we are to convince others of its correctness, that we should have seized all, and not merely some of the points which tell in our favour. It is also of great importance that we should have arranged these in our minds in their logical order, that we should have discriminated between

those proofs which are more and those which are less cogent; that we should have both the facts and the arguments not hazily adumbrated, but clearly and distinctly in our minds. This I have found medical witnesses often fail to achieve.

But with far greater frequency they omit another duty of equal, perhaps greater, importance; namely, to ascertain and impartially weigh those facts and arguments which tell against their own opinion. These disputed cases are never altogether one-sided. We do not perform our duty to ourselves, and we omit the far higher duty of doing justice, unless before even forming an opinion, we have given due consideration to all the elements of the case. We do not perform our duty to our employers unless after having adopted an opinion we seriously reflect upon the best method of supporting it. This includes the best method of answering objections. Were these principles more constantly recognised, we should avoid a fertile source of opprobrium. I do not admit that there exists, except in rare cases, that kind of partisanship on the part of medical witnesses which is sometimes alleged, and more frequently insinuated. I do admit that in common with all mankind a medical witness having enunciated an opinion is prone to adhere to it. The fact that he does so is, I submit, no proof that he has formed it corruptly or even unadvisedly. The *way* in which he supports it, the style in which he meets hostile criticism is, when these are unseemly, the result, in the vast majority of cases, of what may be indifferently termed comparative ignorance or one-sided knowledge. Satisfied, and it is here assumed justly satisfied, of the correctness of his opinion, he has failed to weigh beforehand facts which may be inconsistent, without being irreconcilable with that opinion.

The following is an illustration of the truth of this theory. I was once engaged to assist the counsel of a prisoner, who was being tried for murder, in the cross-examination of the medical witnesses. These gentlemen had made up their minds that a murder had been committed, and that the prisoner was the murderer. I have never seen opinions adhered to with greater tenacity. In spite of overwhelming authorities quoted against them, they would not admit any anatomical, physiological, or surgical dictum which seemed even remotely against the opinion they had, in perfect good faith, erroneously formed. The jury without hesitation brought in a verdict of "Not guilty." It is unnecessary to enlarge upon this, there is such an obvious absence of the motives sometimes insinuated against us, and, indeed, no motive is reasonably conceivable but the one I have suggested.

Facts have come to my knowledge which show that even those who have been frequently concerned in medico-legal inquiries may fail to appreciate the importance, or rather the necessity of understanding

the case they have to meet as well as that which they are going to support.

The Baconian maxim that writing makes an accurate man, suggests to us the remedy for the shortcomings above pointed out.

The case should be fully taken down in writing. This may seem an easy matter. To the few it is so, to the many it is not. One of the greatest existing blots both of medical education and of medical literature is the lamentable and systematic imperfection of case-taking and case-reporting. Habitually and almost uniformly those features only of a patient's case are taken which are supposed from previous knowledge to be pertinent or important. Those, that is to say, which have an obvious connection with the view we take at an early stage of our notemaking of the nature of the disease. Statements which from our prepossession seem irrelevant are ignored, the poor patient, indeed, being often snubbed for taking the liberty of making them. It would be foreign to my present purpose to point out all the disadvantages of such an imperfect method. It is, however, obviously fatal to anything like discovery of new facts, or new bearings of old ones. This is germane to the question before us. Because these cases we are peculiarly bound to investigate in a discovering spirit. Prudent regard to our own interests, as well as common fairness to the patient and to the other parties interested alike demand this of us.

By writing out in full all the facts, all the inferences, and all the arguments, we shall have a chance of weighing them judicially. We shall see what opinions can and what cannot be maintained in the face of hostile criticism. This is a most important matter because, in the ordinary course of practice, we are so in the habit of stating opinions to those who are both indisposed and incompetent to attack them, that we unquestionably contract a loose habit of stating with confidence opinions which will not stand free examination. In ordinary practice they stand well enough, because they are not challenged. In the witness-box we are placed in totally different conditions, and often appear to much less advantage in consequence.

A full written statement has further advantage. It, either in its entirety or in a condensed form, may be presented as a report to those who have the conduct of the case. This enables them to utilise our conclusions in the most advantageous manner. It will not, then, be our fault if we are asked to support views which we do not entertain.

Such medical reports as I have seen, have been, to my thinking, far too meager and scanty for any useful purpose. The one subjoined, though imperfect enough, may, it is hoped, be of assistance to those whom the preceding remarks may induce to adopt the plan which I have myself found useful. It might easily have been dressed up so as to present a better appearance. I have, however,

after much consideration, thought it better to print it exactly as it was made, omitting only all names, and printing in italics every word which did not appear in the original.

REPORT.

Mr. A. B., examined on behalf of the ——— Railway Company, March —th, 18—.

Present. *Five medical men* on the part of the Company, and *two* on part of the plaintiff.

It appears that plaintiff was injured in the ——— accident last June. He was taken, totally insensible, to the ——— Infirmary, and remained there for about seven weeks. There is no doubt that he received severe concussion of the brain, and that he did not recover consciousness for nearly a week after; that during this time he had retention of urine, which required the use of the catheter, and that he had irregularity (*inequality*) of the pupils, showing that one side of the brain was more severely shaken than the other; that when consciousness began to return, it was at first only partial and fitful, and that he suffered from wandering of the mind. It is also to be admitted that he retains no recollection whatever of the circumstances of his accident; it may also be admitted (though I am not quite sure that it is true) that he retains no recollection of the incidents of the journey shortly before the accident.

All these facts indicate that the case was one of ordinary severe concussion of the brain, of which they are the usual and common symptoms. On the other hand, they are not in the slightest degree evidence or symptoms of any secondary, subsequent, or progressive disease or disorganization of the brain, nor do they justify any anticipation that softening of the brain will ensue. Nor are they at all inconsistent, in any case, with complete restoration of the brain power.

But although these phenomena are not necessarily indicative of permanent brain injury, it cannot be denied that, in some such cases, softening of the brain, more or less complete, does ensue. The question then arises—are there any independent evidences that any softening has commenced, or is likely to arise, in this case.

To this question I answer, without any hesitation or reserve,

1st. That there is not a tittle of evidence of any kind that the brain has sustained any permanent injury, or that it is at present undergoing softening in any part.

2nd. That there is no evidence which justifies the assumption that any such disease will arise hereafter. With regard to the first point it is necessary to enter into particulars.

I. After severe concussion, there is no doubt that the brain may either be left enfeebled to a certain extent—an enfeeblement which,

though permanent, is not progressive—or that it may undergo a gradual process of weakening, atrophy, or softening, these three conditions being practically, though not technically, identical.

This condition, which may briefly be described as “softening” (or “ramollissement”), is recognised by some alteration in—

a. The special senses.

b. The power of the mind, as regards attention, reasoning, memory, comprehension, &c.

c. The power of the muscles, *i. e.* paralysis.

a. He did not allege any defection of the special senses. He admitted to Mr. — that his sight was as good now as it was twenty years ago.

He did not allege any alteration in the sense of touch, either increased sensibility (hyperæsthesia), or diminished sensibility (anæsthesia).

b. The power of the mind.

I had the advantage of testing this, not only by my own examination and that of my medical confrères, but also by being present during the whole of his examination before the Commissioner. (*It having been alleged that the man was too ill to bear a journey to the assize where the case was to be tried, the learned Judge sent a barrister, as a commissioner, to take his evidence, the plaintiff being examined by his own Counsel, and cross-examined by one on behalf of the Company; this evidence was afterwards read at the trial.*)

It is important here to note that at the commencement of the medical examination *the plaintiff* persistently abstained from making any attempt whatever to answer questions put to him, replying merely, “that his memory was very bad,” and so on.

He afterwards answered more complicated questions upon the same subjects, without any difficulty of comprehension; and some of the questions were put as tests, to ascertain whether he was determined to make no attempt to answer.

The memory is generally impaired by softening of the brain; and when this is the result of concussion, it may be more impaired with regard to what had happened before the accident, or what has happened after the accident. The latter is by far the more common. It is rarely, if ever, impaired in an equal degree as to the two periods, except when the softening is so far advanced as to be entirely beyond question.

In this case there is no impairment of memory whatsoever, except, as before remarked, with regard to the circumstances immediately antecedent and subsequent to the accident; but this a symptom of concussion, not of softening of the brain. His memory seemed to me to be just as clear about recent and current matters as it did about those of longest date. He made no misstatements, so far as I could judge, except once or twice, when he directly corrected himself. He

remembered his motives for setting out on the journey to ———, and the minor incidents and circumstances connected with it; he even remembered the erroneous ideas which occupied his mind when he first began to recover consciousness; he remembered things which had come under his notice while lying in the ——— Infirmary, such, for example, as that a boy was brought in with a wound in his foot, and of a lady bringing him some raspberries, and when he had taken some, pressing him to take all. Such things occurring since the accident, and when the brain was undoubtedly but half recovered, would have passed away from an impaired memory, even had they been noticed at the time. He remembered his return from ——— *Infirmary*, and recognised *one of the surgeons who was present at the examination* as the person who met him at the ——— station with a vehicle and brought him home. He had no difficulty in recollecting the circumstances of his previous life, and in relating anecdotes of persons whom he had met, &c.

He passed from one subject to another when required with the utmost facility; but he did not exhibit any tendency to ramble or wander from one to another spontaneously or incoherently.

He told us that he frequently lost the thread of his discourse, and forgot what he was going to say on the subject about which he was going to speak. I watched him closely during the whole of the protracted medical and legal examinations (the two lasting about three hours and a half, *one examination immediately following the other*) and on no single occasion did I observe any failing of this kind. When during the legal examination he was requested to pause till the commissioner had taken his notes, he did so, and invariably resumed where he left off, never repeating himself or losing the thread of his observations, or changing the form of his sentence.

Another proof of the clearness of his mind and the accuracy of his memory was that several questions which had been put to him during the medical examination were also put during the legal one and he returned the same answers, though not always in identically the same words each time. At no time did his attention fail.

He complains of not sleeping very well at night, and of his mind then being uncomfortably busy. This cannot be said to amount to a disease or to be any proof of mental disorder or enfeeblement. Most persons who constantly lie in bed sleep ill; and most persons who fail to sleep soundly find that ideas flow through their minds with undue rapidity and some little loss of connection. This is merely a phase or degree of dreaming. Did he get up and take exercise commensurate with his power, there is no reasonable doubt that sound sleep would soon be procured and this dreaminess as soon disappear. Besides, the room in which he sleeps is not conducive to sound rest; it being small and containing two beds, the

air cannot be very pure, though unfortunately I omitted to ascertain how many persons sleep in it.

It seems to me, then, that his present mental condition absolutely negatives the existence of any softening of those parts of the brain which minister to thought, just as the condition of his special senses negatives any softening of those parts with which they are connected.

c. It is possible to have softening of parts of the brain, which are not intimately connected either with the mind or the special senses. Such a condition would produce paralysis, with or without spasm. There is no paralysis, nor have there been spasms. I shall refer to this matter again when speaking of spinal disease; but it may be well to note here that should it be contended that a certain amount of alleged weakness in the legs is of a paralytic nature, and dependent on brain disease, the answer is this. There was evidence (the irregularity—*inequality*—of the pupils) that the original concussion affected the brain more on one side than the other; and did the paralysis depend on such softening, it would be more advanced and perceptible in one leg than the other; but the legs are equal in power, and the contrary has not been alleged.

From these facts, and upon these considerations, then I can come to no other conclusion than that there is no pretence for affirming that his brain is at the present time substantially in a worse condition than it was before the accident, that there is no softening of the brain nor any reason to anticipate it, and that there is not the slightest trace of imbecility. The want of perfect rest at night and dreamless sleep would be at once remedied by his getting up and taking suitable exercise. The same remark applies to a certain degree of headache of which he complains. This he described in the vaguest manner, though strongly pressed to be more precise and definite. I may here remark that he invariably adopted this course when minute inquiries were made regarding any definite symptoms; and his manner during the medical examination strongly contrasted with his manner during the legal one. This *headache*, so far as I could gather, affects the back part of the head, and is, probably, of a congestive character. Mr. — (*his own medical man*), I think, so designated it. This would, unquestionably, be relieved by the erect posture and conversely aggravated. At all events, it is not of sufficient importance to interfere with his ordinary pursuits, and is certainly, *per se*, no evidence at all of softening, or of a tendency thereto.

It is not at all impossible that this pain at the back of the head, and also a pain he vaguely complains of in his shoulder, are of a rheumatic character; the fact that they are worse at night favours this view.¹ Purer air and more exercise would be much more conducive to recovery than the condition in which he was now living.

This view was confirmed by the condition of the urine, which was subsequently examined.

I should add that, during the whole of his protracted examinations, there were no signs of hysteria, which so much excitement would almost certainly have induced in a person with enfeebled mind or softened brain.

II. With regard to any prospect of future softening. It cannot be denied that persons who have had severe concussion of the brain are occasionally liable to softening thereafter, and (as is believed) therefrom, even after a lengthened interval of several months (12-48) of apparently perfect health. In the absence of any present indications of such a process either existing or impending, this, should it be referred to by *his medical advisers*, should be treated as a remote contingency of inappreciable value.

It is most probable that it will be contended on the part of the *plaintiff* that he has sustained "spinal injury and concussion" as well as cerebral. It is not at all improbable that his case may be rested on this chiefly or even exclusively. Should this point not be raised the following considerations may be ignored. It is necessary to consider this question in detail. The term "spinal disease" is often used in a very vague manner, and Mr. — (the *first medical witness to be examined for the plaintiff*) should be pressed to say whether he supposes the disease to affect the muscles, ligaments, or bones of the spinal column, or the membranes of the spinal marrow (cord), or the spinal marrow itself.

a. Membranes of the cord.

It is highly improbable that a fall or concussion without fracture should produce any disease of the membranes alone; there are no symptoms of its existence; there is no pain in the lower extremities when they are moved, and no spasms, cramps, or twitches of them; neither is there any tucking up or contraction of the legs, nor any difficulty in bending them. Pain in the back is not of itself any adequate diagnostic symptom.

b. The cord itself.

An affection of the cord itself under such circumstances would be "softening."

This might be attended with pain at the seat of softening. But it would certainly by this time have produced symptoms of paralysis of the lower extremities, and probably of the bladder; and it would probably have been attended with cramps, pain, or spasm in the legs or thighs. None of these exist, as he himself admits.

c. There is no pain on bending the back, *therefore* no injury to ligaments.

d. There is no pain on pressing the muscles or on forcibly rubbing them, nor on motion, *therefore* no injury to muscles of back.

e. Disease of the spinal column.

He informed us that he had pain in the back, which is a symptom of diseased bones; but it was impossible to get him to be at all precise

as to its seat, or at all to define its character. When disease exists in the bones patients never find any difficulty in doing either of these things.

This indefiniteness is characteristic of muscular and fibrous rheumatism, and this backache, like the headache and shoulder-pains, if these have any existence, are probably of a rheumatic character.

The only definite symptom which I could discern was that he bent himself backward when pressure was made over the last half of the dorsal vertebræ and the upper lumbar. He did this equally whether the finger was passed lightly along the spine, or the pressure was much more considerable, even when he was struck on those parts. He gave vent to no expression of pain, nor did his countenance indicate that he suffered any. There is no irregularity (curvature) or undue prominence of any of these vertebræ.

This single phenomenon is not, and cannot be taken as any adequate proof of any disease of the bones, in the absence of corroboration, for the following reasons :

Disease of the vertebræ very rarely indeed commences, even from injury, after forty years of age ; it is invariably confined at first to one or two—at the most two. The tenderness, irritability, or pain, which pressure or percussion elicit in such a case, is limited to the immediate site of the disease, except, perhaps, when it affects the neck ; it is notably increased by motion, especially by any twisting or bending of the spinal column itself. It is always much more severe upon strong than upon slight percussion. When it extends to several vertebræ, this is only after a very long time, probably years, and is always accompanied by remote symptoms, such as spasm, cramps, twitchings, dragging, or paralysis of one leg or both, or paralysis of the bladder, as well as by curvature of the spine itself. The absence of these has already been mentioned. The following circumstances seem to be absolutely conclusive that there is no disease of the vertebræ.

When I asked him to sit up in bed that I might examine his spine I offered to assist him. Of this offer he took no notice, but raised himself directly without any difficulty or apparent pain (for evidence of which I narrowly scrutinized him), although he was lying flat on his back. No motion could be more difficult or painful to a person with diseased dorsal vertebræ. Before getting out of bed, and when half raised up, and leaning on one elbow in a semi-erect position, his wife proposed to put on his stockings. This she did, not very deftly, and during the process he remained, although there was no necessity for it, in this most constrained position, than which none could have been more painful to a person with disease of the part supposed. He turned first on one side and then on the other, without any apparent difficulty or inconvenience.

Twice during *the legal* examination, becoming somewhat excited by a recital of some old grievances, he assumed a somewhat similar (*semi-erect, leaning on his elbow*) position, and neither time did any expression of pain shade his countenance.

When he got out of bed he walked the length of the room with perfect freedom and alacrity, although *one of his medical advisers* called out to him to be very careful in his movements. He stood by the fire till asked to sit down. He sat till he was told to go back to bed, then rose and walked back to bed with equal ease, and without any appearance of pain or fatigue.

When a man can take a certain amount of exercise without pain, difficulty, or exhaustion, he can do more: there can be no good reason why he should always lie in bed, and make no effort to get about.

I can see no reason why he should not have gone to *the last assize town, a few miles off*, nor much why he should not go to *the next assize town some distance off*.

His muscles are firm and well nourished, wonderfully so for a man who has been so long in bed. His pulse is also good.

On the whole there can be no reason to think that he has any disease of the spine whatever, or that he is in any way permanently injured.

II.—On some points in the Pathology of Cholera and Apnœa.

By GEORGE JOHNSON, M.D., F.R.C.P., Physician to King's College Hospital, Professor of Medicine in King's College, London.

IN the present communication I have passed in review the chief facts which bear upon the pathology of cholera. I have criticised some theories of the disease which have recently been propounded. I have replied to some objections which have been made to my own theory, and have endeavoured to explain some phenomena which have hitherto received no satisfactory explanation. In particular, by a comparison of the phenomena of apnœa with those of cholera collapse, I have explained the blood thickening which is common to those two conditions.

Mr. C. Macnamara, of Calcutta, has recently published an elaborate work on cholera. The chief interest of the book consists in the abundant evidence which it contains of the spread of cholera by contagion. Millions of pilgrims congregate, they bathe in the sacred Ganges and drink its filthy water. Cholera appears amongst

them, and in their progress homewards they scatter the disease over the whole of India, whence it may extend to Europe and over the far west to America.

Mr. Macnamara has obtained conclusive evidence of the presence of a specific poison in the cholera stools. Thus, to use the author's own words, "I may mention the circumstances of a case in which the most positive evidence exists as to the fact of fresh cholera dejecta having found their way into a vessel of drinking water, the mixture being exposed to the heat of the sun during the day. Early the following morning a small quantity of this water was swallowed by nineteen persons (when partaken of the liquid attracted no attention, either by its appearance, taste, or smell). They all remained perfectly well during the day; ate, drank, went to bed, and slept as usual. One of them on waking next morning was seized with cholera, the remainder of the party passed through the second day perfectly well, but two more of them were attacked with cholera the next morning; all the others continued in good health till sunrise of the third day, when two more cases of cholera occurred. This was the last of the disease, the other fourteen men escaped." The author further states that at the time of this remarkable occurrence there was no cholera in the place, none had been there for years, nor has it appeared there since. The only sufferers from the disease were five out of nineteen men, who were known to have once drunk some water contaminated by cholera stools. Here surely we have conclusive evidence that the choleraic discharges contain the specific poison by which the disease is conveyed from the sick to the healthy. We also learn that the symptoms may commence at variable intervals after the poison has entered the system, and that the majority of those who have once swallowed the poison may escape unharmed.

In my opinion the least satisfactory part of Mr. Macnamara's book is that which he devotes to the consideration of the pathology and treatment of cholera. His theory of the disease is very peculiar. He believes that the specific poison having been swallowed does not enter the circulation, but remains in the alimentary canal and there destroys the epithelial covering of the mucous membrane, more especially that of the villi. In consequence of this destruction of the epithelium, there is a more or less rapid drain of liquid from the blood. The "dehydration" of the blood causes collapse. After a time, in favorable cases, the blood by the deprivation of its water having been greatly thickened, coagulates in the intestinal capillaries. This stops the further drain of liquid; then the epithelium is reproduced, and so absorption, which had been rendered impossible during the absence of the epithelium, recommences, the water is restored to the blood, and the patient recovers.

The objections to this theory are numerous and insuperable. I

will mention two or three: First, then, not only is there no proof that the epithelium is destroyed as Mr. Macnamara assumes, but there is positive proof to the contrary. In some cases there is microscopic evidence that a considerable amount of intestinal epithelium is contained in the choleraic discharges during life. This is not constant, and when it does occur it results from a rapid formation and shedding of epithelium by a process of desquamation analogous to that which occurs on the skin and in the kidney in cases of scarlatina. The amount of this desquamation varies greatly both in the cholera and in the scarlatina cases; in some it is very abundant, in others there is scarcely any evidence of its occurring at all. Even when most abundant, the desquamative process neither deprives the mucous membrane of its epithelium nor robs the skin of its cuticle. There is a continual succession of newly-formed cells, the older cells being pushed off by those which are formed beneath them. The denudation of the villi to which Mr. Macnamara attaches so much importance is a post-mortem result of maceration of the mucous membrane in the alkaline contents of the intestines. With respect to this point, I agree with the statement of Drs. Parkes and Gairdner (*'Medical Times and Gazette,'* August and September, 1866). When perfect epithelial moulds of the villi are found in vast numbers in contact with the surface from which they have been detached, the inference is obvious, not only that the epithelium has not been destroyed by the cholera poison as Mr. Macnamara assumes, but that their detachment from the basement membrane in coherent masses has been the result of post-mortem maceration. Then what a strange notion is Mr. Macnamara's, that coagula in the intestinal capillaries having stopped the circulation and the further escape of liquid, yet permit and favour the re-formation of the epithelium! Whence can come the material for the reconstruction of the epithelial cells? Surely not from the stagnant blood in the capillaries. I will presently return to Mr. Macnamara's theory, but in the mean time I wish to direct attention to the manner in which he deals with my theory, to the refutation of which he devotes about twenty pages of his book.

He begins by saying (p. 368), that I take for granted the existence of a blood poison in cholera. Apparently he has overlooked the fact that I devote a whole chapter (*'Notes on Cholera,'* page 65), to "a statement of facts in confirmation of the hypothesis that a morbid poison in the blood is the essential cause of cholera."

Then he says (p. 369, that Dr. Thudichum, "after carefully examining all the structures and fluids of the body in cases of cholera, has arrived at the conclusion that there is no chemical evidence of the presence in the blood of any particular poison." Here we might ask if Dr. Thudichum has obtained chemical evidence of a smallpox or a scarlatina or a typhus poison? and whether in the absence of such evidence Mr. Macnamara doubts the existence of

these morbid poisons. At p. 409 we find the following statement: "We have evidence both chemical and physiological to show that the organic infecting matter of cholera is absorbed into the patient's blood, at all events to some extent, during the deep collapse of cholera, for Dr. Thudichum found the blood at times to contain butyric acid." Are we, then, to believe that the cholera poison is butyric acid? or that where butyric acid is there also is the cholera poison?

And how are we to reconcile this statement with the previous assertion that there is no chemical evidence of a blood poison, and again with another statement (p. 498), that "it is not in our power, and probably never will be, from the examination of the contents of a given specimen of water to determine if the organic matter it contains is derived from cholera products?" He admits that cholera stools contain a specific poison, but he also admits that no examination will enable us to distinguish this from other forms of organic matter. He doubts the existence of a blood poison because Dr. Thudichum failed to find it; he believes in a blood-poison because Dr. Thudichum found butyric acid!

The chief facts which appear to indicate that the cholera poison enters the blood are these: The poison, although in most cases it is swallowed, is sometimes inhaled, and in such instances it must enter the blood before it can reach the alimentary canal. Here there is an analogy between cholera and ordinary diarrhœa. And it is a notorious fact that when diarrhœa has been excited by the inhalation of fœtid gases, the stools have the same peculiar fœtor as the foul air which has excited the disease.

Another proof of blood poisoning in cholera is the fact that in many instances, and perhaps in all, there are symptoms of general constitutional disturbance prior to the occurrence of diarrhœa. These symptoms correspond with those of the initiatory fever of smallpox and other acute exanthemata; and I confidently predict that during this period of blood-zymosis the thermometer will one day be found to indicate an abnormal elevation of temperature.

In most cases of cholera the urine is albuminous. And as in diphtheria, so in cholera, this fact points to the existence of a blood poison.

There are on record many instances, some have come under my own observation, in which the abrupt arrest of the discharges by opiates has been immediately followed by fatal collapse. Thus, MM. Briquet and Mignot state (*Traité pratique et analytique du Cholera Morbus*, 1850, p. 514), that out of 200 cases of diarrhœa admitted into the hospital at the very commencement of the disease and treated by repeated doses of laudanum, no fewer than twenty-six, that is, 13 per cent., passed on into the stage of collapse. My interpretation of these facts is, that the opium, by arresting the

discharges, prevented the escape of the poison and thus permitted its accumulation in the blood. Then followed as a direct consequence an impeded circulation through the lungs and the usual symptoms of collapse. That the opiate treatment of choleraic diarrhœa is not more frequently fatal is due to the fact that in most cases it is powerless to stop the purging until the cholera poison has escaped. The opium must be absorbed before it can act as an astringent, and while there is a rapid rush of liquid from the blood into the bowels, the drug, unable to enter the circulation, is hurried on with the intestinal secretions and eliminated with them. The natural expulsive efforts eject at the same time the morbid poison and the poisonous drug.

One of the most conclusive proofs of blood poisoning in cholera is afforded by the fact referred to in the ninth report of the Medical Officer of the Privy Council, that in several recorded instances the foetus in utero has been killed by cholera, the characteristic discharges being found in the alimentary canal. Now, it is manifest that the foetus could not have swallowed the cholera poison. Before the poison can reach the alimentary canal of the foetus it must have entered the maternal blood and thence it must have passed through the foetal circulation. Here, then, we have an *experimentum crucis*, and those who resist evidence so conclusive must be hopelessly sceptical.

The doctrine that the cholera poison can remain for hours and even for days in contact with the alimentary canal without being absorbed, requires a belief in nothing less than a miracle. The non-absorption of a liquid poison could result only from the suspension of a general law of physiology.

And here I am reminded that not only does Mr. Macnamara believe in this marvellous non-absorption of the cholera poison, but he asserts his belief in an equally miraculous process of absorption occurring *after death*. He admits that the right side of the heart and the whole systemic venous system are found distended after death in the stage of collapse. He says "this depends on the water of the tissues draining into the venous capillaries and right side of the heart after death in collapse, the whole venous system being more or less full of fluid blood from this cause." He says, too, that Griesinger shows that the right cavities of the heart are not full during life, because there is no augmented dulness on percussion over the heart. The question is whether the cavities are full, not whether they are much dilated. Does any practised auscultator believe that he can determine the question of the relative fulness of the heart's cavities during life by percussion?

The only kind of percussion by which this point could be determined would be that which is commonly called "spirit rapping;" and I trust that neither Griesinger nor Macnamara is a believer in the results of that spirited mode of percussion.

That Griesinger should have published such a statement is surprising ; it is still more surprising that any writer should quote it as trustworthy evidence. It is quite certain that during collapse, however rapidly induced, the arteries are nearly empty, and if the veins are not over-full what can have become of the blood ? That the veins are over-full is clearly shown by the lividity of the surface, which is rarely absent in cases of sudden collapse.

Mr. Macnamara, feeling the difficulty of explaining sudden collapse by his theory of epithelial decay and consequent dehydration of the blood, attempts to throw doubt upon the existence of such cases. He says that to describe the attacks as being "so sudden that men fell as if they had drunk the concentrated poison of the upas-tree" is high-flown language. He gives this as an extract from my book, and any one reading the quotation would suppose that the language is mine, whereas it appears in my work as an extract from an official report on cholera in the Black Sea fleet in 1854, and the men who thus fell suddenly into collapse were robust British sailors. But I find in Mr. Macnamara's book language quite as "high flown" with reference to the suddenness of collapse and death. Thus at p. 17, in an extract from the 'Proceedings of the Bengal Medical Board' for 1817, the disease is spoken of as "running its course generally in a few hours, and sometimes in a few minutes." At p. 19 Dr. Corbyn reports that, in 1818, "some of the Governor-General's servants dropped down dead behind his chair." And at p. 24 there is an extract from Jameson's report to the effect that at Punderpoor in 1818 "the patients were described as having been knocked down dead as if by lightning." Surely the lightning beats the upas-tree in speed. Fancy the destruction of the intestinal epithelium, and the consequent dehydration of the blood occurring with the rapidity of a flash of lightning ! It is manifest from these reports that the onset of collapse is sometimes as sudden as an epileptic seizure, and a true theory of the disease must explain these awfully sudden attacks.

Again, Mr. Macnamara, in opposition to almost every eminent writer on cholera, maintains that collapse is never out of proportion to the rapidity and the abundance of the discharges. I have elsewhere quoted many authorities upon this point. I now prefer to quote Mr. Macnamara's own book. In an appendix he gives a report on cholera by Dr. Bruce, and he refers to it as a valuable record. Dr. Bruce says : "It is still a favourite opinion with many that the amount of discharge and collapse stand in the direct relation of cause and effect, but experience is opposed to such a conclusion ; for who has not seen marked cases in every epidemic where there was neither purging nor vomiting, sufficient to cause any degree of collapse, and yet these cases have sunk at least as rapidly as others where the discharges were profuse ? Many of the

cases treated last year, especially in August, were of this description ; the subjects of two of them were amongst the best and steadiest men in the regiment, and whose account of themselves could be fully relied on. One of these men stated that he had one copious fluid motion, after which, feeling unwell, he came to hospital immediately ; he vomited for the first time in my presence as he was taken out of the dooly. I found him in a state of collapse, and he was dead within six hours of that time, having been purged again only once ; this last was a pure flocculent congee motion, but it was only his second ; and in the post-mortem examination an unusually small quantity of the congee fluid was found in the intestines. The other was a case of much the same kind ; he had three congee stools and vomited three times in all." Dr. Bruce also refers to some cases in which, "although the purging and vomiting were checked without difficulty, the collapse steadily increased." Upon this I venture to suggest that the collapse increased *because* the vomiting and purging were checked.

I have explained Mr. Macnamara's theory, and have mentioned some facts which are inconsistent with it. I will now give a brief account of my own theory of cholera, and invite my readers to point out to me any facts which are opposed to it.

I believe that the poison, whether it enters the system by the lungs or by the alimentary canal, is absorbed into the blood, where it excites morbid changes, which are often associated with more or less of malaise and constitutional disturbance. This period of the disease, between exposure to the infection and the commencement of the bowel symptoms, corresponds to the stages of incubation and the initiatory fever in the acute exanthemata. The poison and its products are then expelled by vomiting and purging.

The gastro-intestinal symptoms of cholera correspond with the cutaneous symptoms of smallpox : they are the means by which the poisoned patient is restored to health. Cholera is no more a mere bowel disease than enteric fever is a bowel disease, or than smallpox is a mere skin disease. As in smallpox death may occur from the abundance of the eruption, so in cholera death may occur from the copious discharges and the injury sustained by the mucous membrane during the process of excretion.

When the poison of cholera is abundant or very virulent, or when the process of elimination has been early and abruptly checked by opiates, the blood is so morbidly changed that the minute arteries of the lungs contract and hinder its passage. The systemic arteries now are nearly empty, as shown by the feebleness or complete absence of the pulse, and by the shrinking of the tissues, while the systemic veins are overfull, as indicated by the lividity of the surface. The aeration of the blood is lessened in proportion to the scantiness of the blood stream through the lungs. The lessened

oxidation explains the fall of temperature ; it also explains the suppression of bile and urine, as well as the lessened exhalation of carbonic acid. Carbonic acid, bile, and urine are joint products of oxidation. If a nursing mother has cholera, and passes into collapse, the secretion of a watery milk continues unchecked. The reason is, that the constituents of milk, casein, oil, sugar, and water, are not products of oxidation, while the suppressed secretions, bile and urine, are. The fact of the continued secretion of milk during collapse has been noted by competent observers, from Majendie, in 1832, to the medical officers of the London Hospital, in 1866 ('London Hospital Reports,' vol. iii, p. 449). Mr. Macnamara doubts the fact, because "he cannot understand where the watery elements come from." A fact which he cannot explain should rather make him doubt the truth of his theory than the reality of the fact.

No death from cholera can be too sudden to be explained by this theory of arterial contraction. Nothing can surpass the terrible suddenness of an epileptic fit, and we have now good reason for our belief that the immediate cause of the seizure is the arrest of the cerebral circulation by the contraction of the minute arteries of the brain. Again, the admission of air into the veins, or the injection of certain salts into the veins, may cause a fatal arrest of the pulmonary circulation in less than a minute.

I have elsewhere ('Med.-Chir. Trans.,' vol. l) endeavoured to prove that the immediate cause of all forms of collapse is a defective circulation of blood, and that this defective circulation may be a result of loss of blood or of blood constituents by hæmorrhage or by excessive purging, of cardiac weakness from nervous shock or such depressing poisons as tobacco, or, lastly, of excessive arterial resistance to the flow of blood.

It is obvious that excessive purging must be a source of exhaustion. It is, I think, equally obvious that the main and essential cause of the defective circulation and the collapse of cholera is arterial resistance in the lungs. The cardiac weakness in cholera is a secondary result of the scanty stream of blood through the coronary as through other systemic arteries, and the loss of liquid, which, when profuse, is undoubtedly a source of exhaustion, often bears no direct relation to the degree of collapse, the symptoms of which it is quite inadequate to explain.

I now wish to direct particular attention to the state of the blood in cholera. The blood drawn from the veins during the collapse stage is black, tarry, and of high specific gravity. Why is this? Because the water has been drained off by the bowels, say some pathologists. But why, then, does not profuse diarrhœa, which is not associated with collapse, thicken the blood? Because, it is said, during collapse absorption is suspended, and the blood, therefore, cannot recover the water which it is losing. I will presently show

that this explanation, though incomplete, contains an element of truth; but first let me remark that there is no relation between the amount of discharges and the degree of blood thickening. A patient who passes into collapse with little or no discharge has his blood as much thickened as one who has lost a large amount of liquid. Then this blood thickening is not peculiar to cholera. It occurs in other diseases which are not associated with watery discharges from the blood. Dr. Dundas Thompson, who published an elaborate paper on the chemistry of the blood in cholera ('*Med.-Chir. Trans.*,' vol. xxxiii), states that a specimen of blood from the nose of a patient who laboured under an affection of the mucous membrane of the air-tubes, contained a greater excess of solids in proportion to water than he had found even in cholera blood.

Dr. Buchanan (now Professor of Surgery at the University of Glasgow), in a pamphlet on tracheotomy in croup and diphtheria, states that "As soon as the wound was made [in performing tracheotomy] a quantity of dark, tarry-looking blood flowed from the edges, and was with difficulty cleared away by firm and repeated applications of a moist sponge." He afterwards suggests that this viscid and tarry condition of blood may "be one of the pathological elements of diphtheria."

I shall presently show that this condition of blood is a result of blood stasis in the veins consequent upon apnoea. We know that in cases of laryngitis or diphtheria, threatening death by apnoea, if the operation of tracheotomy be too long deferred, not only does the stagnating blood in the whole venous system become black and viscid, but it may actually coagulate in the vessels and in the right cavities of the heart.

Now, what is there in common between these cases of apnoea and cholera collapse? Obviously not a drain of liquid, but a partial arrest of blood in the minute branches of the pulmonary artery.

Let me invite attention to the following description of a disease, whose name I will for the present withhold:

"The oxygenation of the blood is so imperfectly performed from the sparing supply of air, that it is inadequate to the maintenance of the normal temperature. The extremities especially get cold, and blue, and shrunk. I have known the whole body deathly cold, and resist all efforts to warm it for four hours. But while the temperature is thus depressed, the perspiration produced by the violent respiratory efforts may be profuse, so that the sufferer is at the same time cold and sweating. It is this union of coldness and sweat, combined with the duskiess and pallor of the skin, that gives to the patient so much the appearance of a dying man, and that even sometimes makes the initiated fear that death is impending. The pulse is always small, and it is so feeble sometimes that it can hardly be felt."

Here we have the description of a state of collapse closely resembling the collapse of cholera. It is, in fact, an extract from Dr. Salter's description of a fit of asthma. Dr. Salter goes on to say that the smallness of the pulse is the result of a partial arrest of blood in the lungs, and this again is consequent on an imperfect supply of air. As soon as the bronchial spasm relaxes, the pulse resumes its normal volume.

In the second edition of his book Dr. Salter accepts my explanation of the arrest of blood in cases of apnœa, namely, that it is caused by contraction of the minute pulmonary arteries, and not by an obstruction in the capillaries, as was formerly supposed. (See on this point a paper by the author "On the Influence of the minute Blood-vessels upon the Circulation."—'Med.-Chir. Trans.,' vol. li.)

In both cholera and asthma there is impeded breathing and impeded circulation. In asthma spasm of the bronchi keeps back the air from the blood; the blood stasis is a secondary result; in cholera spasm of the minute arteries keeps back the blood from the air, and the apnœa is consequent on this.

The post-mortem condition of the lungs and of the heart's cavities, as regards the amount of blood which they contain, is precisely the same in acute apnœa as in cholera collapse. The right cavities, the pulmonary artery, and the whole venous system, are distended, the left cavities are empty, the minute tissue of the lung is nearly bloodless. The blood has been arrested in the minute branches of the pulmonary artery, and the contraction of their muscular walls affords the only possible explanation of the arrest.

In a paper in the 'Lancet' (June 20th, 1868) I have shown that the dark colour of the lungs in some cases of collapse and of acute apnœa, which gives them a congested appearance, whilst their diminished weight and their dryness are proofs of their containing very little blood—this dark colour is simply a result of engorgement of the *bronchial* veins and capillaries in common with the whole systemic venous system.

The best account of the condition of the lungs and the heart in cholera is that by Dr. Sutton, in the fourth volume of the 'London Hospital Reports.' Mr. Macnamara did not weigh the lungs, therefore his post-mortem results are of little value.

Now, I have shown that a partial stagnation of blood in the venous system is common to cases of apnœa and to cholera collapse. I have also shown that blood thickening is common to these two conditions. Why is this? First let me repeat that loss of water by purging, however copious it may be, does not thicken the blood; and I will state the reason. The soft tissues of the body, the muscles, the viscera and the areolar tissue, &c., contain four fifths by weight of water; and as water passes out by the bowels, water

passes through the capillaries of the various tissues into the veins, so that the volume and the liquidity of the blood are maintained by this compensatory absorption.

So rapid is the absorption of water, that the blood which last flows during the operation of venesection has a lower specific gravity than that which first escapes from the open vein. (This has been proved by Dr. John Davy and by Polli. See 'Kirkes' Physiology.)

The rapidity with which absorption occurs in the subcutaneous tissue is shown by the quick and powerful action of morphia and other narcotics when given by subcutaneous injection.

Again, see what happens in a case of dropsy. A dose of elaterium acts as a hydragogue. As the water escapes from the blood through the bowels, the blood-vessels recoup themselves by sucking up the dropsical fluid from the areolar tissue and the serous cavities, and the dropsy is sometimes rapidly removed.

Sir Thomas Watson mentions the case of a man in whom a profuse purging, excited by intoxicating doses of rack-punch, led to the disappearance of a large hydrocele in the course of a night. Here it is clear, as Sir Thomas remarks, "that the expenditure of serous liquid from one part led to its absorption into the blood from another." This compensatory process of absorption explains the fact that profuse watery purging, without obstruction to the pulmonary circulation, does not sensibly increase the viscosity of the blood; and this compensatory process has been entirely overlooked by those pathologists who have made nice arithmetical calculations of the amount of water that must escape from the vessels in order to render the blood as viscid as it is in cholera.

In these calculations the arithmetic is right, but the physiology is wrong. The calculations are based upon the erroneous assumption that the blood-vessels have impermeable walls, and therefore have no power of borrowing liquid from the neighbouring tissues—an error somewhat analogous to that which would be implied in calculating the time required to empty a room of air by estimating the volume of rarified air rushing up the chimney from the fire, and taking no account of the influx of air through the crevices of doors and windows.

The diminished weight of the kidneys and spleen after death in collapse is probably in part due to the interstitial absorption of water, and in part to the defective circulation during collapse.

Now at length we come to the explanation of the blood thickening in collapse. It is clear from a comparison of cholera collapse with apnoea that the blood, in order to maintain its normal composition and its liquidity, must move freely, and be freely exposed to the influence of the air. Slowly moving, imperfectly oxygenized blood becomes dark and viscid; and stagnant blood, even within the

vessels, slowly coagulates. As the watery part of the stagnating blood gradually separates from the solid constituents, it may escape in two directions. The whole venous system being distended, some serum may passively transude through the capillaries into the areolar and other tissues, and some perhaps through the skin, constituting the cold sweat of cholera collapse and of asthma; so that when the circulation through the lungs becomes obstructed, not only is absorption of liquid from the tissues prevented, but the distension of the systemic veins may lead to an escape of some water from the blood into the tissues. On the other hand, a larger proportion of the serous than of the more solid constituents of the blood may, perhaps, be permitted to pass on through the resisting arterial stop-cocks in the lungs, and thus a more viscid blood is left behind in the veins.

There is yet another consideration which probably in part explains the increased density of the blood in cholera collapse and in apnœa. So long as the blood is imperfectly oxygenized, there will be an accumulation of unoxidised materials which, with a free circulation and respiration, would be rapidly oxidised, and converted into carbonic acid, bile, and urine. I hope I have made it clear that the blood thickening in cholera is a consequence, and not the cause, of the impeded circulation through the lungs.

I have long ago discovered that it is quite useless to discuss the treatment of cholera with a man who has a wrong theory of the disease. Mr. Macnamara believes that the cholera poison has a purely local action upon the epithelium of the alimentary canal. One would suppose that this theory would suggest the expediency of ejecting the poison by some mild yet quickly acting laxative; but, on the contrary, Mr. Macnamara does his best to "confine the enemy within the bowels," by half-drachm doses of laudanum during the diarrhœa stage. When he declares that this is a successful plan, he will, I hope, forgive me if I express myself utterly incredulous.

He has little hope from any plan of treatment during the stage of collapse. Although I differ from him as to the means to be employed, I agree with him that it is much easier to prevent collapse than to bring a patient out of that fearful condition when once he has passed into it. I know too much of the physiology of collapse to expect great results from any method of treatment in that stage of the disease.

I have just received from India a pamphlet on Cholera, by Mr. George Barnard, Staff Surgeon of the Indian Army, who declares his belief that cholera is "a specific acute inflammation of the mucous membrane of the small intestines," and that a grain of tartar emetic every quarter of an hour is so certain a remedy for the disease, that "in future it will amount to criminality on the part of

a practitioner of medicine to withhold it." This gentleman evidently does not see that the mere fact of some of his patients having recovered from cholera after taking fifty (and in one case seventy-eight) grains of tartar emetic in the course of a few hours, affords conclusive evidence that they could not have been suffering from muc-enteritis.

I can imagine Mr. Macnamara's horror at the thought of villi, which he believes to be deprived of their epithelial covering, having to "bide the pelting of this pitiless storm" of tartar emetic.

Happily in most cases these large doses of antimony would go the way of large doses of laudanum. The rapid current of liquid from the blood through the bowels prevents their entrance into the circulation, and they are quickly washed away with the poisonous discharges.

In the second volume of the 'Transactions of the Clinical Society' Dr. Murchison has published two papers, which are apparently intended to illustrate the pathology of cholera. In the first paper he gives the history of a fatal case of cholera in a woman, whose symptoms of collapse were for a time much mitigated by a saline injection into the veins of lower temperature than the blood. Dr. Murchison infers that "the marvellous effects of saline injections into the veins in the collapse of cholera cannot be due to the fluid injected being of a higher temperature than the blood in the branches of the pulmonary artery."

I am not aware that any one has attributed the relief from the saline injections *entirely* to the warmth of the fluid. I have always maintained that it has a twofold action. The fluid as such "mixing with the blood in the pulmonary artery would, as it were, dilute the poisoned blood and render it less irritating, just as diluents render the urine less irritating to an inflamed bladder or urethra" ('Notes on Cholera,' p. 62).

Dr. Murchison's injection had at the commencement a temperature of 100° , but as no means were adopted for keeping it warm, he assumes that it would soon become cooler than the blood. Now, it is to be observed that the blood in the superficial veins of a patient in collapse is several degrees below the normal point. Professor Czermack, of Vienna, found that 88° Fahrenheit was, in round numbers, the average temperature of the blood drawn from the veins of seven patients in collapse, after allowing for the loss of temperature in passing from the vein into the cup. (See Briquet and Mignot, 'Traité Pratique et Analytique du Cholera Morbus,' 1850, p. 282.)

Notwithstanding Dr. Murchison's conclusions from his single case, I still believe that the warmth of the injection has a powerful influence in diminishing arterial resistance, and for these reasons;—men who have had much larger experience of saline injections in

cholera declare that hot injections are more efficacious than tepid fluids.

Dr. Mackintosh, in the year 1832, injected 156 patients, of whom 25 recovered. The temperature of his injections varied from 106° to 120° F., and he states that "the good effects of the injection were rapid in proportion to the heat of the solution." ('Practice of Physic,' vol. i, p. 365.)

Mr. Little injected 20 cases at the London Hospital in 1866, of whom 5 recovered. Mr. Little's injections had a temperature of 110°, and he states that "a temperature below 100° is decidedly injurious." ('Med. Times and Gaz.,' April, 1867.)

Many observers have found that external warmth, immersion in a warm bath, packing in a hot wet sheet and blankets, often improves the character of the pulse. This it does probably by warming the venous blood on its way back to the heart. These facts surely render it probable that the temperature of the saline injections is not without influence; and until Dr. Murchison has succeeded in saving at least one life by injections of lower temperature than the blood, he can scarcely expect to make many converts to his explanation of their mode of action.

I have never denied or doubted that the mere addition of liquid to the blood may be of use, by supplying the vehicle for removing the poison and its products from the system; but this does not explain the almost instantaneous benefit resulting from the hot saline injections into the veins.

Dr. Murchison's second paper is "On a Case of Gastro-Enteritis from local irritants simulating Cholera on two occasions in the same individual." On the first occasion "pungent decayed cheese" was the exciting cause of the disease, and on the second occasion American lobster. On both occasions there was vomiting, much purging, collapse, suppression of urine, and, after reaction, albuminous urine.

It is not quite clear whether Dr. Murchison attributes the collapse to the local irritation of the mucous membrane, as seems probable from the title of his paper and his reference to Mr. Sedgwick's paper in the 'Medico-Chir. Trans.,'¹ or whether he attributes the collapse to the loss of fluid by vomiting and purging. My own conviction is that it was mainly due neither to the one nor the other, but to blood poisoning.

¹ Mr. Sedgwick believes that the cholera poison does not enter the blood, but has a local action upon the nerves which supply the mucous membrane of the stomach and intestines. He compares the collapse of cholera with the collapse which follows perforation of the stomach or intestine, and the consequent escape of irritating liquids into the abdominal cavity. Apparently Mr. Sedgwick does not see that the nerves and tissues here affected are not those of the mucous membrane, but those of the peritoneum, from which by the very conditions of his theory the cholera poison is excluded.

The temporary albuminuria is, to my mind, conclusive evidence of blood poisoning.

In a note Dr. Murchison refers to the cases of a mother and three children who were admitted into the fever hospital with symptoms of gastro-enteritis after eating decayed American cheese; one child died in collapse, and the mother died in convulsions. The mucous membrane of the stomach and intestines was found red and inflamed in both cases, and pieces of cheese were found in the intestines. "Nothing was found in the brain of the woman to explain the convulsions, but the kidneys were slightly granular." Is it not highly probable that the collapse in the boy and the convulsions in the woman were due to an interrupted circulation through the lungs in the one case, and the brain in the other, and that this interrupted circulation was caused by the cheese-poison in the lungs? The boy died of pulmonary epilepsy; the woman of cerebral epilepsy. It is an instructive fact that, although both patients lived two days after the seizure, yet *portions of the poisonous cheese were still found in the intestines*. Dr. Murchison does not tell us what treatment was adopted in these cases, but the fact referred to shows that evacuants, rather than astringents, were called for.

Dr. Christison, in his work on poisons, refers to numerous cases of poisoning by various articles of food in a particular stage of decomposition. In most of these cases there were signs of gastrointestinal irritation, and in most, too, there are the clearest indications of blood poisoning. With reference to some cases of poisoning by pork, he says :

"Stimulants, opiates, and bloodletting are of no avail; and the only useful remedies are emetics and cathartics, which speedily put an end to the symptoms by removing their cause" (Christison, p. 649).

In 1826, on the coast of Galloway, four adults and ten children ate a stew made with meat from a dead calf which had been found on the sea-shore. For three hours no ill effect followed; but they were then all seized with pain in the stomach, efforts to vomit, purging, and lividity of the face, succeeded by a soporose state like the stupor caused by opium; except that, when roused, the patient had a peculiar wild expression. One person died comatose in the course of six hours. The rest, being freely purged and made to vomit, eventually got well (Christison, p. 647). In these cases the formidable symptoms were clearly due to blood poisoning.

During the discussion on Dr. Murchison's paper one speaker is reported to have referred to a case of poisoning by half a drachm of croton oil. Sickness and purging followed, and the symptoms were those of cholera collapse. Pereira mentions a case in which two and a half drachms of croton oil caused collapse in three quarters of an hour, in an hour and a half vomiting and purging, and death, with symptoms of asphyxia, in four hours. It can scarcely be doubted

that, in these cases, the worst symptoms are due to blood poisoning, and not to the vomiting and purging. In a case of poisoning by croton oil liniment, referred to by the editors of Pereira, the patient was seized with convulsions, probably because the oil had reached the cerebral arteries. And in a case quoted by Pereira himself a man, who had been poisoned by *inhaling the dust of croton seeds*, became giddy, insensible, and collapsed. In this case it is clear that the symptoms were the result of blood poisoning. There was epigastric pain, but no purging, and therefore, on the following day, several doses of castor oil were given to him. My object in referring to these cases is to suggest that the collapse in these and similar instances is due, not to the vomiting and purging, but to the interrupted circulation which results from the introduction of the poison into the blood.

Notwithstanding the rapidity with which castor oil acts as a purgative, and is expelled from the bowels, we have proof that a part of it becomes absorbed, in the fact that usually when a nursing mother takes a dose, some of it enters the milk and purges the infant.

The following case is a striking example of blood poisoning by lobster.

My friend Dr. E. L. told me that some years since, when he was living with the late Dr. Brinton, he awoke one morning with urgent dyspnœa. After a time the dyspnœa went off, and again returned. Dr. Brinton was much alarmed at his appearance. After a while flushes of heat came on in the skin, and at length an eruption of nettlerash. As soon as the rash appeared the dyspnœa finally ceased. He had no vomiting or purging, but, remembering that on the previous evening he had eaten curried lobster, he took a dose of opening medicine. He had often before eaten lobster without inconvenience, but Brinton suggested that the stomach of the lobster might have contained some jelly-fish, and that this had got into the curry. In this case we can trace the poison from the stomach to the venous blood, through the lungs, where it caused dyspnœa, to its final exit by the skin.

If the poison, finding an outlet by the alimentary canal, had excited vomiting and purging, a pathologist who disbelieves in blood poisoning might have attributed the pulmonary distress to the drain of liquid from the blood.

Tartar emetic affords a good illustration of a poison which when swallowed exerts a local irritant action on the mucous membrane of the alimentary canal, causing vomiting and purging, while at the same time a part of the poison is absorbed, and thus exerts a peculiar depressing influence upon the nervous system. When I gave tartar emetic as a medicine more frequently and freely than I now do, I observed that the peculiar depression and nausea did not occur

when the medicine acted quickly as an emetic and purgative: the reason being that the nervous depression results from the absorption of the antimony, while vomiting and purging tend to prevent absorption and to facilitate the speedy elimination of the drug.

Dr. Taylor remarks that, when in cases of antimonial poisoning vomiting and purging are absent, "the symptoms affecting the nervous system are generally more prominent" ('On Poisons,' p. 537). He relates (at p. 538) two cases of poisoning by a large dose of tartar emetic. In both there was extreme collapse within an hour after the poison was swallowed, then succeeded, for several hours, profuse watery purging, and as this continued the state of collapse gradually passed away. In one case there was a copious flow of urine and pain in passing it, and "on the third day the whole of the body was covered with genuine tartar emetic pustules." Here we have conclusive evidence of collapse from blood poisoning, and of elimination by the skin and kidneys, as well as by the alimentary canal.

In these facts we have, as I think, an obvious analogy with the absorption of the cholera poison and the relation of the gastrointestinal symptoms of cholera to those of collapse.

No one doubts that a diarrhoea may cause great exhaustion, or that it may be so profuse as to be fatal; but if we bear in mind the rapidity with which all poisons—animal, vegetable, and mineral—enter the circulation and thus pervade the whole system, we shall beware of attributing to the drain of liquid from the blood the formidable and fatal symptoms which result from blood poisoning. And we shall take heed lest, in our attempt to arrest the discharges, we shut in the self-multiplying morbid poison which is their exciting cause.

It appears to me that one lesson to be learnt from a study of the mode in which cholera originates and spreads is this:—It is at once the interest and the duty of England, both by precept and by example, to give sanitary instruction to her two hundred millions of Hindoo subjects. Obviously it is right to adopt measures for stamping out cholera, and for preventing its spread from one part of India to another, and from India to Europe. Obviously, too, if cholera is to be finally extirpated, we must teach the natives of India that the only safeguard against this disease is to be found in cleanliness; that the drinking of foul water, however sacred may be the stream from which it comes, is always dangerous and, in epidemic seasons, fatal; and that so long as loathsome impurities which have once been thrown off from the body are permitted to re-enter the system, either with air or water, the scourge of cholera will continually recur. Lastly, it is the duty of government to afford all possible facilities for effecting a thorough drainage of towns and villages, for obtaining a sufficient supply of pure water, and for preventing the admixture of sewage with drinking water.

III.—An Inquiry into the Real Nature of Hysteria.

By D. De BERDT HOVELL, F.R.C.S.E., &c.

(Continued from p. 211.)

SINCE the first portion of this paper was written a lamentable instance of the evil arising from the habit of designating a depressed condition of physical and moral power by the inappropriate name Hysteria, and attributing the attendant disabilities to wilfulness, obstinacy and deceit, has just occurred in the case of Sarah Jacob, who, with equal incorrectness and misapplication of terms, was called "The Fasting Girl in Wales." The expedition of nurses devised by metropolitan and provincial physicians was not only founded on a misconception of the real state of the case and a waste of medical power, but also an entire misdirection of treatment. It seems to have been undertaken with the idea that to convict the patient of imposture was a fundamental and necessary step towards her cure.

It failed grievously in respect of both objects. It certainly proved, what every sensible person knew before, that the girl could not, and did not, live without food. But it did nothing towards removing the physical and moral disability of anorexia or inappetency which constituted one real difficulty of the case.

If, instead of setting four experienced nurses to watch a weak, ignorant girl starve to death, one sensible motherly woman had been appointed who would have undertaken to put food in her way, and induce her to eat overtly instead of covertly, the result of the case would most probably have been very different. The post-mortem examination revealed lumbrici in the ileum, which at once accounts for many of the symptoms, and especially for the disinclination to take food. As regards the patient, the condition of ill health appears to have been brought on by loss of blood in the first instance, and moral depression and loss of moral power ensued upon physical. The case would come under Division II, according to the arrangement of this paper. The commonly received opinions concerning this class of case are well represented by the letter of the senior physician of the hospital which provided the nurses to a leading public journal,¹ and the remark has been completely verified that "The term Hysteria refers to a condition of ill health with which every one is familiar, but which nobody pretends perfectly to understand," &c. "No medical man of experience," he writes, "would regard such a case as that of the Welsh Fasting Girl with any interest whatever. Foolish and mischievous juggling of the kind is common enough."

Let me ask, can any class of case be devoid of interest of which

¹ 'The Times,' December 25th, 1869.

the real nature is so undetermined and misunderstood? If juggling exists in such cases, the more need of a clear exposition by competent persons; and if physicians decline to investigate and explain, who is to undertake the task? The poor child was not the cause of the juggling, but the victim of it. The existing prejudice on the subject, and the superstitious interpretation of it, were fostered by the ignorance and credulity of the neighbourhood. It is a mistake to stigmatise as wilful and intentional the deviations from moral rectitude to which patients in this state are liable; they are morbid products just as much as the cutting of a man's own throat is the result of depressed spirits and ill health, during which a right principle of action is lost sight of. No good is done by thrusting a stake through the dead body and burying it in a crossway in the one case, or by branding the living character with falsity and imposture in the other. The object of treatment should be to relieve the condition which leads to such painful results, not to punish those who are unfortunately the subjects of it. If such cases were rightly diagnosed in an early stage, the physical disabilities removed, and the moral condition elevated, certain good would be done and much evil prevented. It is because the term *Hysteria* is associated with so much prejudice and so many erroneous views that it is so desirable, if not actually necessary, to extirpate it. If the condition were recognised to be essentially that of want of power and loss of strength, both moral and physical, there would be a greater probability of the right help being afforded. We should have the satisfaction of sound and clear opinion instead of vague and obscure hypothesis, and a greater probability of treatment being adopted more worthy of a liberal and enlightened profession.

"In ordinary conversation," says Dr. Tyndall, in his 'Lectures on Sound,' "the physical precedes and arouses the psychological. The spoken language which is to give us pleasure or pain, which is to move us to anger or soothe us to peace, is a purely mechanical condition of the intervening air." The different emotions thus produced are quite distinct, it is to be observed, from the perceptive power or faculty receiving the sound which gives rise to them. It is not only through the sense of hearing, in the case of sound, that these effects are produced, but through that of sight, in that of reading. The emotion follows with equal rapidity in both instances, and although the communication is received by an organ of special sense in either case, by the ear in hearing, and by the eye in seeing, the effect does not dwell with the percipient or intellectual faculties, but falls upon those of feeling. We experience a familiar instance of, not only the optical impression but also the sensation of, motion, if when sitting in a railway carriage at rest, another train begins to move gently at the side. For a short interval of time we are doubtful whether *we* are moving or the train beside us, and it frequently

becomes necessary to correct our sensation by the observation of some fixed object. In this case the mental perception and the sensation, although quite distinct in themselves, pass the one into the other so quickly, that they become confused, and apparently identical.

These are instances of moral effects produced in our systems by physical causes; but the class of cases more immediately under investigation represents the converse of these, viz. the physical effects upon our systems of emotional or psychical causes. The precise pathology of this is difficult to arrive at, partly because by far the greater proportion of cases is temporary only, and in the small number that go on to a fatal termination the immediate effects are usually beyond the reach of morbid anatomy. Add to which that the results are not simple, but complicated with other maladies; not only so, but the physiology of the subject is also quite undetermined, and this in itself constitutes a great, if not, indeed, a main difficulty, on the very threshold of investigation, inasmuch as a knowledge of the normal or natural condition is indispensable before seeking to explain abnormal or morbid changes.

Inquiry into the subject carries us not only into the comparatively unexplored regions of Psychology, but into the "terra incognita" of Metaphysics. It may thus become useful in elucidating the one, and leading to a better definition of the other; for we are obliged to consider, separately, the functions of the body, the directing power of the mind, the regulation of both by the feelings, the ruling influence of character or principle in directing all; in a word, body, *σώμα*; mind and feeling, or that combination of the mental and moral which constitutes the soul, *ψυχή*; and the principle or spirit, *πνεῦμα*.

It here becomes necessary to distinguish the material conditions of our system or constitution from the immaterial, and, as far as is practicable, to determine both, as well as the still unsettled question of the inherent qualities and forces of matter, and vital force or principle. Some qualities or forces have so long been found in connection with matter, that they have come to be regarded as essential qualities. It is possible, however, that the difference between intrinsic and extrinsic forces may not have been perfectly considered. Professor Huxley considers that "the assumption of a special vital force is illogical and unscientific" [Physical basis of life]. "The mechanical actions of the body are governed by the same laws which regulate similar changes of energy in the organic world. Where matter is, there force must be, showing itself in motion, in tension, or in resistance. Without matter no kind of motion, nor tension, nor resistance occurs." "When hydrogen and oxygen," says Mr. Huxley, "are mixed in certain proportions, and an electric spark is passed through them, they disappear, and a quantity of water, equal in weight to the sum of their weight, appears in their place." Here

it is evident that the imponderable electric spark is not an intrinsic power or force appertaining to oxygen or hydrogen, but an extrinsic force necessary to their conversion from a state of gas to a state of water. Thus, to all intents and purposes, the electric spark may be considered as a "special vital force" essential to the production of water out of certain proportions of oxygen and hydrogen.

It is the province of the physiologist to investigate the laws and conditions of the healthy structure and functions of the body; of the pathologist to explain the morbid changes and alterations of both body and function; and he cannot have failed to remark the different condition of matter which is the seat of vital force, and that which is devoid of it. "It is now known to everybody," says Dr. Tyndall, "that the vegetable world is nourished by the rays of the sun, and as animal life is sustained by vegetable, so that life also is supported in the long run by solar rays." Science should be telescopic as well as microscopic. It is thus clear that the quality of matter which appears to be inherent must be considered in the condition in which life is absent, as well as that in which life is present. The material conditions of the dead body are quite different and distinct from the material conditions of the living body. The organic forces in both cases are not identical, any more than the condition of the vegetable when nourished by the rays of the sun, and existing independently of them. If the relation between the solar rays and the vegetable were defined it would be difficult to meet with a better term than that of "special vital force," which Mr. Huxley has pronounced to be illogical and unscientific." And

"What other fire than he
Whereby the blood beats, and the blossom blows,
And the sea rolls, and all the world is warmed."

Now, what the solar rays are to vegetable life in the first case, and to animal life in the second, what the electric spark is to the formation of water out of certain proportions of oxygen and hydrogen; that is the character, or principle, or spirit, to the physical condition of man. There is a distinct line of demarcation between his material and immaterial condition.

The question at issue is simply whether the constitution of man is altogether material or, in some respects at least, immaterial also; whether the forces which act upon his constitution are essentially organic or material; or whether some, at least, are inorganic and immaterial; whether the material part of his nature has an independent existence; or whether it does not exist independently of his immaterial part in the same way that vegetable and animal life does not and cannot exist without the vitalizing influence of the solar rays. "If mind be but a function of matter," says Mr. Lecky in his work on rationalism—"if thought be but a material product of the brain,—it seems natural that the dissolution of the body should be the

annihilation of the individual." The brain is undoubtedly the organ of perception, intelligence, mind, and in a similar way, as regards its material condition, the tongue is the organ of speech; no perception, thought, or reflection can take place without molecular change in the condition of the brain; so far these are, strictly speaking, functions of the brain; and with a damaged brain we have, as a necessary consequence, corresponding damaged function. Equally so is the tongue the organ of speech; no speech or utterance can take place without exercising the function of the tongue, and if the machinery of the tongue, including, of course, its nervous distribution, be impaired, speech is impaired also; but speech is no more the be-all and end-all of the intellect, than the intellect is the be-all and end-all of the individual; and as speech is directed by the intellect, so the intellect is influenced by the moral powers and the higher principles of man's nature.

The body of man is material; "his breath goeth forth—he returneth to his earth in *that very day his thoughts perish*." Yes, his body dies, and his *thoughts* perish, but his character and his name live. David is dead, and Solomon is dead, but the principles which are handed down to us in the writings of both still survive to influence us, if we choose to adopt them. The same may be said of Plato, Aristotle, Lord Bacon, Lord Macaulay, and Sir Walter Scott; so also of Cæsar, Hannibal, Napoleon, and Wellington. Their deeds are past, their brains have ceased to act, but the memory, aye, and the influence of no one of them has altogether been lost. And "that noble fellow" Admiral "Collingwood is dead," but the "gallant way in which he brought his ship into action" still lives in our minds and hearts; it is not an organic force, or a vital force, but it moves us, elevates us. We neither saw it, nor heard of it when it was done; but we have read it in books, and "our fathers have told us." We have received it through our organs of special sense, and the effect of it has fallen upon our organisation and influenced it.

It has thus been my object to draw a distinction between the intellect, as a material part of man's nature, and the principles of action or immaterial part, which influences that as well as his moral qualities. It may be objected that the character of an individual is the association of his mental and moral qualities, and that the component parts being material, the combination and product is material also; it may also be urged that the character is evident, and partakes of the nature of the individual who is material and mortal; still the fact remains that principles of action and character survive the mortal part of the individual. The immaterial becomes dissociated from the material and survives it.

Midway, as it were, between the intellect, which, in round terms, may be considered to represent the organs of sense, and the prin-

ciples or ruling influences which preside over our whole organisation lie the moral powers of the passions and emotions, which again, in round terms, may be said to represent the organs of feeling. Although these last are not only in conjunction with the two former, but closely associated with them, both in co-operation and antagonism; they have a separate existence, and, like the intellect, are essentially material. The feelings, passions, and emotions die with the body, but the intellectual and moral part of our nature, which, associating with our principles of action, forms our character, survives. Like our whole being, they remain material or become immaterial, according as they are influenced or not by extrinsic immaterial influences; and as the vegetable and simply animal body has no life without the solar rays, so the material body without the spirit is without life also. The assertion that the passions and emotions—in other words, our moral qualities—so far as they remain purely physical, are material, will probably be received with incredulity by some; but it is clear that they, like the thoughts, must perish when the “breath goeth forth.” The admission of this fact is necessary to the right understanding of the subject which is more immediately under consideration, because the class of disease which is sought to be elucidated essentially represents the *physical effects of moral and psychical causes*.

Having called attention to the distinction to be drawn between the intellect and the emotions, and between both as material conditions and the immaterial principles which, although ruling and influencing our whole nature, have an independent existence, it becomes necessary to examine into their properties and modes of acting more in detail. Purely intellectual action neither excites nor depresses; in fact, does not *move us*. This is the property of the emotions, as their name implies. The passions are closely associated with them. Strictly speaking, they represent disturbing influences under which the individual upon whom they act is suffering or enduring, under which he may remain passive, or be aroused to action. Their nature proclaims them to be powerful agents. The peculiar effect of a principle is to elevate, to set in action, or to confirm a mode of action already begun. Thus a principle of action is essentially positive in its nature. But there are negative conditions, the effects of which are diametrically the reverse of a positive action. Of these the most familiar, as well as the most comprehensive, is Disappointment. Some degree of vague misconception has arisen on this subject from want of accuracy in distinguishing principles from emotions. Hope and fear are frequently spoken of as being antagonistic. Hope is an immaterial principle—Fear, a depressing emotion. Fear is not, as its frequent antagonistic conjunction would imply, the exact antithesis of Hope, the condition induced by its absence or deprivation. The loss of hope induces disappointment in the first instance

but the “*Lasciate ogni speranza*” would be properly represented by Despair.

The effects of emotions are not only different, according as they are pleasurable or painful, favorable or adverse; but they are varied and different in themselves. Take, for instance, the effects of Fear, or rather of Horror: “*Obstupui, steteruntque comæ, vox faucibus hæsit,*” which may be thus translated:—I was dumb-founded, my hair stood on end, my voice stuck in my throat; or, to speak physiologically, my intellectual functions were temporarily suspended, preternatural action was excited in one respect, and a voluntary action suspended in another. Take, again, an instance of a more distinct loss of physical power, an actual paralysis, or rather paresis, indicated by the phrase, “I stood rooted to the ground.” We must pause one minute to observe the difference between simple and compound emotions; for instance, fear is simply a depressing emotion; in Horror there is a degree of revolting element, which, although it does not excite reaction, prepares for it. Anger and Indignation represent not only excited action, but tension, and preparation for resistance. So Worry, and, in its aggravated form, Anxiety, represent not only depressing influences, but a condition of irritation superadded. In all these, it is to be observed, the feelings are principally acted on; the intellectual functions become involved, but they are not *primarily acted upon*. Thus it would appear that it is not the brain which is acted upon, but a portion of the nervous system which regulates the intellectual, and motor powers both voluntary and involuntary. Pursuing this inquiry further, we find that the effect of emotions falls immediately upon the organs of circulation, that we have increased and perturbed action of the heart; flushing of the face or pallor; a warm glow or a cold sweat, as the case may be; and we come to find that not only the organs of circulation, but those also of secretion are affected; and not only that Worry affects the secretions, but that the continuance of it in the graver form of anxiety affects pathological changes, which are even more important.

Pausing for one minute to notice the distinction between the mere increase and decrease in the action of the circulating system, and the disturbance thereof; between the increase and decrease in the secretions, and the disturbance of those functions; between simple increase and decrease of growths, and those perversions of structural changes which constitute diseases more or less malignant; to call attention to the effects of emotions which are pleasurable or adverse, merely exciting or depressing, and those, on the other hand, in which tension and irritation give rise to disturbed and perverted action, and glancing for one moment at the probable relation and connection which exists between the two as cause and effect, we pass on to a more close consideration of the physiology and pathology

of our subject, taking for granted the truth of the assertion that—(1) The cause of emotion is apprehended by organs of special sense; (2) communicated by them to that portion of the nervous system which they principally affect, thus producing (3) certain physical effects. It has been before mentioned that the precise effect on the nervous system is difficult to arrive at, partly because it is indirect and complicated, partly because it is difficult to determine the functional or organic change in any part of the nervous system so affected. It is one of those subjects in which we have greatly to rely upon a comparison between normal action and abnormal effects, and thence to draw our conclusions.

It would be a great omission not to quote the researches of Dr. J. Lockhart Clarke, into “the structure of the olivary bodies, and the inferences drawn by him as to their uses.” They had formerly been regarded as having control over speech, expression, and deglutition. Upon anatomical and pathological grounds he regards them, not only as centres through which the different movements are co-ordinated for expressing passions and emotions, but considers that they are the motor centres through which the *different movements are effected* by sudden, violent, or peculiar impressions on the special senses; for they are intimately connected with all the sensory ganglia of the medulla, and have been traced by him nearly to the perforated space (‘Brit. and For. Med.-Chir. Rev.,’ No. lxxxvii, p. 75). I beg leave to call especial attention to this conclusion, that “the centres through which different movements are co-ordinated for *expressing* passions and emotions, and the motor centres through which different movements are effected;” in other words, that they are the ultimate rather than the immediate portion of the nervous system on which emotions fall, and that this opinion is quite consistent with that which I am about to advocate—that the sympathetic or vaso-motory system is that principally affected by emotions.

The following passage, quoted from a monograph by Dr. Daldy on ‘Diseases on the Right Side of the Heart,’ bears closely on this subject:—“Why emotional disturbance should produce in one person excited and frequent heart-action, and in another slower, and, as we call it, depressed heart-action, is a problem not easily solved. The association between an abiding cause of mental (?) anxiety and deterioration of the heart’s muscular structure will be admitted by every observer whose attention has been directed to the subject.” Dr. Daldy then proceeds to mention the case of a lady who died literally of a “broken heart,” rupture of the left ventricle having taken place at the apex, and the pericardium having consequently been found full of blood. He then proceeds to say:—“I suppose it will be admitted that the due nutrition of the heart-tissue depends upon the integrity of its ganglia.”

Mr. Hilton, “in the course of his dissections many years since,

met with a heart perfectly developed on the left side, but attenuated and imperfectly developed on the right. The apparent cause of this condition proved a minute and prolonged inquiry to be that the ganglia of the right side were shrivelled and imperfect, those of the left being healthy."

I would here observe once more of Anxiety, that although the effect falls upon the mind, it is essentially a moral and not a mental disturbance in the first instance. It would hardly be correct to designate a weapon by which the brain was injured by the term mental or intellectual; to speak of a mental bludgeon would be absurd.

The state of ill health under consideration is very common, but at the same time anomalous and obscure; it has been difficult to reconcile the clear intellectual condition which existed in one patient with the unaccountable perversity which prevailed in others; the inability to move which prevailed at one time, with the capable activity that was evident at another; the freedom from pain at one time, with its occasional violent and spasmodic return. It was evident that neither the mind nor the motor powers were permanently affected, but that some "*tertium quid*" was concerned which held sway over both. Many considered the state to be unreal and fictitious—

"That though it *seemed* to be a fit,
'Twas nothing but a feint."

But the disease sometimes occurred in patients whose previous character was deemed incapable of deception, and in such the morbid state was unworthy of the patient. How was the anomaly to be explained? The experiments of M. Claude Bernard threw a sudden and unexpected light on the subject; they showed the alteration in the function of the sympathetic consequent on division by the knife, namely—1. Dilatation of vessels with increased rapidity of circulation. 2. Increase of muscular irritability, and generally increased sensibility of the cerebro-spinal nerves, constituting hyperæsthesia; in other words—

1. Loss of control over the circulating system. 2. Loss of tone in muscular power. 3. Loss of tone in the nerves of sensation.

As a consequence of these conditions we should expect to meet with—1. Paresis, loss of power manifested in various ways. 2. Hyperæsthesia, undue sensibility, and liability to pain. 3. General susceptibility to irritation from various causes.

The cervical and cranial ganglia of the sympathetic communicate with (1) the spinal nerves, (2) those of special sense, and (3) give off branches accompanying arteries, especially the carotids. Thus the sympathetic will be observed to be connected with (1) excito-motory action, (2) special sense, (3) and to exercise a special function of its own over vaso-motory action and secretion. Considering also the influence which it has in regulating the sanguineous circulation,

which is essential to the nutrition of the nervous system, it may also in a higher sense be regarded as essential to—

1. Motion. 2. Reflection. 3. Feeling and Affection.

There is also a still higher sense in which it comes to be regarded as an element necessary to the exercise of the physical functions, by means of which we exert and call into play—

1. Voluntary Action. 2. Reason. 3. The Passions and Emotions.

This brings us to the moral part or condition of our nature, which has the chief power of control over our physical and mental condition, and which it has been before stated lies, as it were, midway between the intellect and the principles of action which govern both.

Starting once more from (1) Volition; (2) Reason; (3) Passions and Emotions; we arrive at (1) Will; (2) Intellect; (3) Sympathy; which lead up to (1) Hope; (2) Faith; (3) Charity; which last are undeniably immaterial principles.

Courage may be regarded as a quality which is the preventive and corrective of fear; Resolution as the exercise of a power re-collecting the elements of a condition which has been displaced, loosened, or fallen to pieces: it is an undoubted exercise of Will. Determination implies a conservative exercise of Will, prior to the condition of depression consequent on adverse circumstances. Courage must be considered a material quality; Resolution and Determination as immaterial principles, called into play in order to neutralize some opposing or depressing agent, consequently they are both allied to Hope. "Confidence is said to be a plant of slow growth, and easily blighted;" it is a principle of trust, grafted and grown upon experience of satisfactory qualities and conduct; it is a principle of action founded on Reason and Faith, and a more intelligent condition than mere determination. Thus :

	Medulla.	Brain.	Sympathetic.
Physical . .	Excito-motory action.	Perception.	Vaso-motory control.
Mental . .	Motion.	Reflection.	Sensibility or affection.
Moral . .	Voluntary action.	Reason.	Passion and emotion.

	Body.	Mind.	Feeling.
Physical . .	Volition.	Reason.	Passion & emotion.
Psychical .	Will.	Intellect.	Sympathy.
Immaterial .	Hope.	Faith.	Charity.

At first sight it may appear incorrect to trace Will from excitatory action through the different degrees of motion and voluntary action, then reinforced by determination and resolution up to Hope; but a moment's reflection will show that some object connected with Hope is the prime mover wherever Determination is called into play. Hope not only "springs eternal in the human breast," a never-failing well-spring bright and pure, but is independent as well as indigenous. Faith, on the other hand, presupposes that the object on which we place it, the being on whom we rely, has been *proved* to a certain extent by reason. Our experience of the past has been such as to induce us to place Faith for the future. Faith without reason is simply credulity.

But, to come back to our more immediate subject, the object of these remarks is to show that the physical condition of man, when depressed by unfavorable emotional influences, and not upheld by higher ruling principles or influences, experiences a paresis, or remission of strength and nerve-power, equivalent and similar to that produced by division of the sympathetic nerves by the knife; that the supply of nerve-power *fails* in the one case, and is *cut off* in the other.

Practically we admit the influence of the immaterial principles which influence our conduct, in our estimate of the characters of different men, the tone of this or that society, and we familiarly speak of an individual whose conduct is devoid of principle as being "abandoned," that is to say, "derelict," the sport and prey of bad habits and evil passions. We have lost faith in him, our hope is lessened, and if we have not ceased to have Charity, it is only because it "never faileth." Again, we cannot fail to recognise a Principle in the Dominion of Man over the beasts of the field. Last, but not least in the Subjective condition of Women, which Mr. John Stuart Mill has lost sight of, but cannot disprove. On the other hand, we must not omit to recognise the effect of physical causes on our moral condition, and ultimately on our principles of action. The first effect of alcoholic stimulants is upon the circulation, then upon the intellect, then upon physical and moral control—of the tongue, the ideas, the conduct. It is the loss of moral control which chains the habitual drunkard to the vice and liquor he detests, but from which he cannot abstain; and whatever proclivity he originally may have had in that direction, all the evil effects of that bad habit are mainly attributable to physical causes in the first instance.

Before proceeding to inquire into the morbid conditions, which are the result of adverse emotional influences and the loss of upholding principles of conduct, that come more directly under the province of the physician, it is not irrelevant to notice the effect of these favorable and unfavorable influences upon the character and conduct of prominent persons with whose character we are familiar.

Determination and disappointment represent two opposite conditions ; the one implies power and tension, the other loss of power and relaxation of the Will. They are not only antithetical, but, as it were, the antipodes of one another. They do not so much constitute conditions of the nervous system as they represent states which are well-marked instances of the effect of powerful action on the one hand, and the loss of it on the other.

The different and opposite conditions thus produced are well represented in the history of Cardinal Wolsey. Contrast his haughty and arbitrary conduct in the plenitude of power urged on by the

“ Vaulting ambition, which o’erleaps itself
And falls on the other,”

and his broken condition after his fall. Ipswich and Oxford and the domains of Hampton Court did not suffice to satisfy him ; but now he is abject and supplicates :

“ An old man broken with the storms of state
Is come to lay his weary bones among you ;
Give him a little earth for charity.”

Read, again, the character of Lord Grey de Wilton as portrayed by Mr. Hepworth Dixon in ‘ Her Majesty’s Tower,’ vol. ii, p. 25 and p. 84 :—“ Grey brought into the patriot’s camp not only a soldier’s sword, but a statesman’s thought ; not only a dauntless eye, but a clear and resolute mind. He knew not merely how to fight, but how to turn the tide of battle to a righteous end. He saw “ not only what should be done, but how it should be done.” “ Look here, upon this picture, and on this.”

“ The old, old story comes up again. They broke his health, and when they had ruined his health, they easily broke his heart. The man who could not be induced to beg for life was worn into begging fretfully for such poor freedom as the liberties of the Tower.” *Proh, Pudor !*

Contrast, again, the calm confidence of the successful Duke of Wellington with the despondency and depressed nerve power of his equally brave and skilful, but less successful, predecessor Sir John Moore ; and finish the painful story of disappointment and crushed hopes in the eccentric conduct and blighted life of the high-spirited and noble lady who “ Never told her love.”

We may truly link the glorious death of the Hero of Corunna with the voluntary exile and solitary existence in the arid desert. There is something very painful in the account of the flighty interview between her and the author of Eöthen.

We might well exclaim, as in the case of Hamlet, “ Oh, what a noble mind is here o’erthrown !”

But we must not overlook the altered conduct of persons under

these circumstances. It is about the last last thing we should expect of a high-born and intellectual woman accustomed to move in the best and highest circles of society that she should adopt male attire—that she should

“Lay aside
Her native modesty, and ride astride.”

“There is something in this more than natural, if philosophy could find it out.”

It is universally admitted that the character and conduct of Mary Queen of Scots requires some explanation beyond that given by history. This, in some measure at least, may be afforded by Psychology. The loss of her first husband, the disappointment which must necessarily have been caused by her second; his violent death; the cool audacity of her third; her removal from “la belle France,” which was so congenial to her habits and temperament, to the uncongenial climate of the land of her birth—uncongenial, not only in its physical but in its social atmosphere. The difficult position of a beautiful woman, without much moral support, in the midst of rude men, especially when that beautiful woman was the centre and pivot of so many plots and schemes, matrimonial, political, and religious, occupying, as it were, the field between two rival crowns, and two intensely opposed religious denominations, two rival countries, in the very hotbed of fierce religious controversy. No wonder if, beset on all sides with difficulties and misfortunes, and unsupported by healthy influences, subjected to rude shocks, such as the murder of Rizzio in her presence, her conduct in some respects should have deviated from the high standard which was natural to it.

“Then gently scan your brother Man, and gentler sister Woman,
Though they may gang a kenig wrang, to step aside is human.”

It is indeed well to remember this important feature in the class of cases under investigation. Persons who are hard and thick-skinned are not obnoxious to the influences which tend to it; the acutely feeling, thin-skinned, sensitive persons are most amenable to them. It is not in some cases so much that the *want* of *spirit and determination* leads to it, as that the *absence* of *feeling* exempts from it. Even the Iron Duke on one occasion succumbed to the influence of disappointment, and became so unfit for his duties that he returned home from India.

We hear frequently of persons brooding, giving way to their troubles, hugging their griefs, moping, being despondent, blue-devilled, and so becoming confirmed hypochondriacs; that it is a pity they do not rouse themselves, that surely they have had sufficient time to recover, and so on. Very true; still it may be that those who speak thus, without realising the state, may lose sight of the fact that the condition is something more than one of mere de-

pression, that it is an actual loss of physical and moral power, that "a broken spirit drieth the bones." "The heart knoweth its own bitterness;" and there is much in the condition of these patients that may be passed over in a superficial view. "The *spirit* of man will *sustain* his infirmity; but when by sorrow of the heart the spirit is broken, the *wounded spirit* who can bear?"

It comes, then, to this, that the condition under investigation is a valid one; that it is not fictitious or fanciful; not wilful, or actually depraved; but that physical power, and moral power, and control are lowered; that the altered condition thus induced is capable, and, indeed, highly susceptible of aggravation from irritating causes; that these causes, which are invisible in themselves, become clearly seen in their effects. We may even go so far as to say that the causes are in many instances not only invisible but immaterial; but they nevertheless find a material expression in the physical symptoms which they produce. In some instances the converse of this obtains, and our character or immaterial condition is influenced by causes purely physical, fatigue, or the shock of an accident, for instance. It is also clear that so long as the effect of that cause prevails, or is kept up by irritating and aggravating causes, or indirectly by the absence of right treatment, this state may continue, and even be prolonged indefinitely; thus it may last eight years, as in the case quoted above, or it might go on for eighteen, for the matter of that; but the result of that case clearly showed that when the right treatment was adopted, intentionally or unintentionally, the patient recovered, became well in health, and resumed her place in society. It is also quite clear that a right understanding of the nature of the case on the part of the physician was the essential thing that was needed, rather than a want of conformity on the part of the patient. It need not be a matter of surprise that with an imperfect knowledge of the nature of the case, and, if the truth be told, some leaven of prejudice, many should have fought shy of such cases, and shirked inquiry into them. They must have been tiresome, tedious, and unsatisfactory.

"But by mine eyes and by mine ears I swear,
I will be deafer than the blue-eyed cat,
And thrice as blind as any noonday owl,
To holy virgins in their ecstacies,
Henceforward."

"Deafer
and blinder unto holy things;
Hope not to make thyself by idle vows,
Being too blind to have desire to see."

IV.—On Animal Vaccination.¹ By P. M. BRAIDWOOD, M.D., &c.

THE term "Animal Vaccination" is applied to the process of propagating the virus of *spontaneous* cowpox through successive generations of healthy heifers, and to the subsequent vaccination of the human subject with this lymph. This method of obtaining vaccine lymph has been pursued in Naples during nearly sixty years. Instituted there by Dr. Traja, in 1810, animal vaccination was continued after his death by Drs. Galbiati and Negri; it was introduced into Paris about five years ago by Dr. Depaul; into Berlin by Dr. Pissin, in June, 1865; into St. Petersburg by Dr. Fröbelius, in 1867; and into Brussels by Dr. Warlomont, in 1865. This process has already received so favorable a reception from foreign governments, that in Italy, where vaccination is compulsory, while vaccinating from "*arm-to arm*" is the method ordinarily employed, two or more institutions have been established for the purpose of supplying heifer's lymph when desired. The French, Belgian, and Russian governments, have each sanctioned similar establishments in Paris, Brussels, and St. Petersburg, respectively, and branch institutions for the same purpose are being commenced in other cities of those countries. It is in consequence of dissatisfaction with ordinary, humanised vaccine lymph, that animal vaccination has been introduced into these Continental countries.

Mode of Operating on the Heifer, and its Effects.—The heifer to be employed should be a healthy animal, from six weeks to three months old. Before being operated on, it is tied down to a table by means of strong, padded leather straps, which, while restraining the movements of the animal, do not injure it in any way. One strap round the animal's neck fastens its head, while its fore limbs are fixed by means of a second strap, and a girdle keeps its body from moving. In St. Petersburg two assistants are employed for holding fast the animal's hinder limbs, and allowing its abdomen to be well exposed. Dr. Blanc, who lately practised animal vaccination in London, made use of only one assistant, and fixed one hind limb to the operating table, while the other was secured by means of a rope to the ceiling of the room. The next procedure is to shave carefully the lower half of the heifer's abdomen. The animal is inoculated by making 120 to 200 punctures or incisions linearly, penetrating the cuticle only. On the Continent a lancet, with a triangular point containing a groove, is employed both for the vaccination of the heifer and of children. This lancet is charged with lymph, and is inserted by puncture just below the cuticle. This mode of

¹ The following remarks on "Animal Vaccination" are based both on a study of the literature of the subject, and on personal observation of the methods now actually pursued in Paris, Brussels, and St. Petersburg.

vaccinating allows of greater rapidity than when scratches are made. After the heifer has been inoculated, a flannel bandage is applied around the abdomen in order to prevent the animal from injuring the vesicles. The heifer is fed after the operation on the food to which it has been accustomed. If the animal suffer from diarrhœa, the addition of rice to its milk will generally restrain this disorder; but more commonly there is a tendency to constipation, which is corrected by the administration of meal. During the first two or three days after inoculation the vesicles are not observable; about the close of the third day, they can be felt to project slightly; on the fourth, they contain a minute quantity of lymph, and they become fully developed on the two succeeding days. Vesicles of the fourth day after inoculation are employed by the vaccinators in St. Petersburg for the inoculation of the next heifer; and vesicles of the fifth and sixth days, for the vaccination of children. In the Foundlings' Hospital of that city, 100 to 200 children are vaccinated daily from the heifer; and here the process of animal vaccination and its results can be studied most conveniently. At this establishment four heifers are kept always ready for the purpose of vaccinating children, and as many as twenty heifers per month are inoculated. The children are vaccinated by six punctures, generally three on each arm. Vaccination by "incision," by "scratches," or by "punctures," is equally successful, in the inoculation of the heifer and in the vaccination of children.

Advantages ascribed to "Animal Vaccination."—The lymph of spontaneous cow-pox, after having passed through the system of a heifer, or of a series of heifers, is held by some to excel ordinary "humanised" lymph, because of the more perfect characters presented by the vesicles thus produced, and which are, therefore, believed to afford a greater protection against smallpox; because of the impossibility of transmitting disease through this source; and because in this way a larger supply of vaccine lymph can be obtained, especially on an emergency, than by the "arm-to-arm" process.

The assertion that the lymph employed in animal vaccination produces better vesicles and a more perfect protection against smallpox involves the supposition that the ordinary vaccine has undergone deterioration. Our first duty, therefore, is to define a *typical* vesicle of the eighth day, when it has attained its perfection, and then to compare with this vesicles derived from the two sources in question. A typical vesicle "is plump, round, and more decidedly pearl-coloured; it is distended with clear lymph; the elevation of its margin and the depression of its centre are more marked. At this date, or sometimes a few hours earlier, a ring of inflammation, termed the areola, begins to form about its base. The areola is circular, and when fully developed, has a diameter of from one to

two inches; it is often attended with considerable hardness and swelling of the subjacent connective tissue.”¹ Such, according to Dr. Seaton, are the characters for which a vaccinator is to look when judging of the results of his operation. These are the appearances presented by vesicles developed by animal vaccination, except in one particular, viz. the extent and constitution of the areola. The “ring of inflammation” surrounding a vaccine vesicle appears to me to be the indication of an effort made by a healthy system to get rid of the results of a local irritant, and therefore ought not to be looked upon as “the anatomical evidence that the cow-pox has produced its specific effect on the constitution.”² It is the secretion of the lymph which I regard as affording this anatomical evidence. The areola resulting from vaccination with heifer’s lymph is, on the eighth day, less extensive, radiating from one quarter to three quarters of an inch around the vesicle, of a bright pink colour, and is unaccompanied by hardness of the subjacent connective tissue. The larger and the more indurated is the areola, the more irritative, but not necessarily the more effective, is the action of the vaccine lymph. On the other hand, after vaccinating with heifer’s lymph, this inflammatory ring is, on the tenth or eleventh day, often of larger extent than when humanised lymph has been used, and may occupy one half of the circumference of the arm. It is accompanied by more or less induration, but this disappears generally about twenty-four hours after it has attained its maximum; whereas, after vaccination with ordinary lymph, the areola requires longer time for its complete disappearance. On examining carefully by the touch, arms which have been vaccinated by these two methods, and comparing them, I have always been struck with the more elastic feel of the areola on the arm of a child vaccinated with heifer’s lymph than of the areola induced by humanised lymph; and this difference cannot be better expressed than by saying that the former variety of areola is like the base of a healthy acute abscess, while the latter resembles the base of a subacute or chronic abscess. Some experienced vaccinators regard a large areola on the *eighth* day after vaccination as a favorable sign of a successful operation; and were this the true view, ordinary lymph would have the advantage over heifer’s lymph. But, accompanying such inflammatory action, the vesicle very frequently appears opaline, the lymph is of a yellow colour and very fluid, or looks muddy; whereas, the vesicles induced by heifer’s lymph are (on the eighth day) surrounded by scarcely any areola, are beautifully clear and transparent, and have a pearly lustre; and the lymph procured from such vesicles is less in quantity, flows less readily, and is of a crystalline appearance. Further, vaccinia induced by heifer’s lymph runs its course more slowly than does the disease generated by humanised lymph. On this account the former may be

1 Seaton, Ed. C., ‘A Handbook of Vaccination,’ 1868, p. 68.

2 Ibid.

regarded as producing a more gradual effect on the constitution, and thereby affording a more permanent protection against smallpox.

Lastly, the appearance of the cicatrix distinguishes successful vaccination. A typical cicatrix is "circular, somewhat depressed, foveated or indented with minute pits, and sometimes radiated. A well foveated cicatrix is, next to having watched the vaccination through its course, the best test we have of the security of the system against smallpox.¹ The cicatrices following vaccination with heifer's lymph are usually of this class, the very kind to be desired. But on searching carefully for such cicatrices after vaccination with ordinary lymph, I have seen them only in a small per-centage of cases. Well-formed transparent vesicles, enclosing clear limpid lymph, surrounded by little inflammatory redness, and leaving characteristic cicatrices, are the results of vaccination with heifer's lymph. While such results, according to our observation, invariably follow animal vaccination, they are much more rarely met with after vaccination with humanised lymph.

In the next place it is necessary to adduce proof that vaccination with heifer's lymph affords a better protection against smallpox than does vaccination with humanised lymph. The evidence just cited favours this opinion. From the limited extent to which animal vaccination has been practised compared with vaccination with ordinary lymph, it is impossible to collect statistics of the mortality from smallpox amongst those vaccinated according to these methods respectively. Nor do statistics showing the diminished mortality from smallpox after vaccination appear to me to guarantee the inference that vaccination with ordinary lymph is as protective as might be desired. If numbers could be produced to prove that smallpox *attacks* fewer persons after vaccination than after variolic inoculation, or that it is better warded off by vaccination with ordinary lymph than by animal vaccination, we would be placed in a position to draw a fair deduction. But, in examining the mortality after smallpox now-a-days, as compared with that twenty years ago, we have to make allowance for the better knowledge of the treatment of fevers in the present day as compared with what it was then. It is undeniable, that many instances of smallpox are still met with, that epidemics of smallpox still break out, even in countries where vaccination has been legally enforced, and that smallpox attacks children only a few years after their protection by means of vaccination.

Additional proof of the greater influence of heifer's lymph over the human economy, as compared with ordinary lymph, is furnished by the results of revaccination with heifer lymph. Dr. Warlomont, of Brussels, gives the following results of revaccination with the two kinds of lymph. Among 924 individuals, between seven and twenty years old, bearing good evidence of a previously successful vaccina-

¹ Seaton, *Ibid.*, p. 69.

tion, revaccination with heifer lymph succeeded in 211 instances, *i. e.* in 22·83 per cent.; while in the case of 277 individuals, from ten to twenty years old, likewise bearing good vaccinal cicatrices, revaccination with ordinary lymph succeeded 9 times, *i. e.* in 3·24 per cent. Again, 6749 Neapolitan soldiers were revaccinated with heifer's lymph, and among these 1670, or 24·74 per cent., had true vaccinia.¹ Still further evidence of the success of revaccination is afforded by a glance at the following table extracted from Dr. Seaton's 'Handbook of Vaccination.'²

Persons in whom the revaccinations were performed.	Degree of success of re-vaccinations.	In those who bore marks of previous small-pox.	In those who bore good marks of previous vaccination.	In those who bore doubtful or imperfect marks of previous vaccination.	In those who bore no marks of previous vaccination or smallpox.
		Per 1000	Per 1000	Per 1000	Per 1000
Wurtemberg army, 1831-5 (13,861 cases).	Perfect	319·5	310·4	280·7	337·3
	Modified	248·1	280·5	259·0	191·1
	None	432·3	409·2	460·4	471·6
Soldiers in British army, not recruits, in 1861 (2053 cases).	Perfect	451·4	484·6	236·8	326·0
	Modified	195·6	157·4	505·0	277·5
	None	389·0	358·0	257·9	396·5
Recruits in British army in 1861 (4395 cases).	Perfect	345·5	407·3	461·3	527·3
	Modified	266·8	240·8	301·3	202·6
	None	387·7	351·9	237·4	270·1

I have revaccinated individuals, varying from eleven to twenty-four years of age, both directly from the heifer and from a child's arm. My opportunity for so doing has been very limited, but it has sufficed to convince me that heifer lymph is more efficacious than ordinary lymph when thus employed. From the preceding statistics we may, I think, draw two inferences, viz. that vaccination does not impart a permanent protection against smallpox, and hence that revaccination is advisable; and also that heifer's lymph, when employed for revaccination, is attended with greater success than follows revaccination with ordinary vaccine lymph. If, then, heifer's lymph is more efficacious than humanised lymph when used for revaccination, especially for the revaccination of individuals who have

¹ Dr. Warlomont, 'De la Vaccination Animale et de l'utilité des Revaccinations à tous les âges de la vie.' Bruxelles, 1865.

Of his revaccinations with heifer's lymph he states that—

180 children were 9 to 12½ years old, and of these 63 showed beautiful vesicles.

200	„	„	7 „ 9	„	„	20	„	„	„
40	„	„	7 „ 13	„	„	8	„	„	„
33	„	„	under 20	„	„	13	„	„	„
71	„	„	14 to 20	„	„	31	„	„	„

In 406 cases the age is not stated, but among these 76 „ „ „

² Seaton, E. C., 'A Handbook of Vaccination,' 1868, p. 279.

been vaccinated successfully with ordinary lymph only a few years previously, we may conclude that this tends to show the deterioration of ordinary lymph and the greater protective properties of heifer's lymph.

It is not supposed that the deterioration of vaccine lymph is due to the mere number of beings through whom the matter has been transmitted, provided the transmission has been effected in a proper manner; but to the careless performance of vaccination which has frequently occurred, either from not selecting good vesicles, or from taking the lymph at an improper time. If pure vaccine lymph, derived from spontaneous cowpox directly or indirectly through a heifer, is propagated carefully through a well-selected series of human beings, we may safely say that it does not lose any of its effective properties—does not degenerate in consequence of such transmission. Dr. Jenner believed that vaccine lymph could be made to pass through a lengthened succession of human beings without undergoing any alteration, and the experience of the last fifty years confirms this presumption. On the other hand, if vaccine lymph, a most delicate, organic fluid, has once been allowed to undergo pathological changes in the vesicles,—has been permitted, for example, to remain in the vesicle till the ninth day, or even later, after vaccination, when suppuration has commenced to alter its composition,—can it be supposed that it has undergone no deterioration? And if such lymph is used for vaccination, is it reasonable to conceive that the passage of this fluid through a child's constitution is sufficient to restore its purity, and to supply any protective power it may have lost? It is generally admitted that vaccination has been hitherto very carelessly performed in this country—that vaccine lymph has been removed from vesicles which were undergoing suppurative changes—that vaccination has been performed with lymph from imperfectly or improperly developed vesicles; and yet it is affirmed that if such lymph be carefully employed, hereafter it will produce results identical with those of the true virus, will be equally protective with the original cowpox. According to this view, the system of a healthy child is made to serve the part, not only of a searching filter, but of a reproducing spring. If vaccination has been performed in this country, so that one half of those vaccinated are not in reality benefited by the operation—that, in fact, 50 per cent. of those vaccinated present imperfect cicatrices,—it is presumable that the “ordinary lymph” (not only the lymph used in certain parts of the country, but indirectly all the vaccine lymph employed) has suffered from this inattention to the necessities of safe vaccination. Instead, therefore, of insisting that, when this imperfect lymph is passed through the system of a healthy child, it becomes pure vaccine lymph—identical with the virus derived from the cow—it would surely be better, scientifically more consistent, to return to the original source of vaccinia, and in place of employing the virus of

spontaneous cow-pox, which is found to act severely on the human system, to use heifer lymph, which "has all the qualities of that of the spontaneous disease, except its too frequent acrimony." By renewing vaccine lymph in this way, we admit that the lymph at present employed is imperfect, not in consequence of a continuous transmission through successive human generations, but that its efficacy has been impaired by a careless propagation of it in the human subject.

Results obtained by Animal Vaccination.—The only experimental inquiry into animal vaccination as yet undertaken by any scientific body, was that carried out by a committee of nine medical men chosen by the Academy of Medicine in Paris, and at whose disposal 6000 francs were placed. This committee prosecuted their researches during 1866, and published their report in the following year. They state the objects aimed at by them in making this inquiry to be two-fold. "In the first place, the effects of the employment of lymph from two sources of spontaneous cow-pox were carefully tested by experiment, the preservation of genuine lymph by transmission from heifer to heifer was thoroughly investigated, and the impossibility of transmitting syphilis to the bovine species was proven. In the second place, experiments were made to show the comparative advantages of vaccination with heifer lymph over arm-to-arm vaccination."¹ The lymph of spontaneous cow-pox was derived partly from M. Négri's establishment in Naples, partly from an instance of the natural disease which occurred in Beaugency. With these forms of lymph 41 experiments were made. In order to test the comparative merits of vaccination from the heifer and of arm-to-arm vaccination, 71 experiments were carried out: 681 children were vaccinated with heifer lymph, 897 children with humanised lymph. With the object of investigating the communicability by vaccination of syphilis to the bovine species only 3 experiments were made by the committee; but the report refers to other experiments, having a similar aim, made by private individuals.

Animal vaccination has been pursued on an extensive scale in various Continental cities by individuals; and the aggregate experience of these several vaccinators forms no slight evidence in favour of this practice. Dr. Lanoix informed me that, from December 1864, to January 1869, he had inoculated 2000 heifers, had resorted thrice to the original cow-pox, and had revaccinated 60,000 persons. No bad result followed any of his operations. Dr. Warlomont, of Brussels, told me that, during March, April, and May, 1869, he vaccinated 2000 children, making six punctures on each arm; in each case twelve vesicles were produced, and not the slightest accident followed the operation. The Continental animal vaccinators

¹ 'Expériences faites à l'Académie Impériale de Médecine avec le Cow-pox ou Vaccin. Animal.' Compte rendu adressé à son Exc. M. le Ministre de l'Agriculture, du Commerce, et des Travaux publics, par M. Depaul, 1867.

affirm that no untoward result has ever followed the operation in their hands. They all agree in the following conclusions, which are best given in the words of the 'Report' of the Parisian Committee, viz. :

"The transmission of cow-pox by inoculation from heifer to heifer is accomplished without difficulty.

"No animal experimented on by us has experienced any accident which may legitimately be referred to the operation.

"The successive transplantation of the lymph has not appeared to influence the character of the vesicles, seeing that those on the heifer last inoculated presented the same appearance and the same dimensions as those on the first.

"The progress of the eruption in heifers is a little more rapid than that of the vaccinal vesicle in man.

"According to our experiments syphilis is not inoculable in individuals of the bovine species.

"Whenever heifer's lymph has been used under proper circumstances as regards age, success has been almost always constant, and the results have been as good as when humanised lymph was used.

"The vesicles obtained by vaccination with heifer's lymph have, in our experience, exceeded in size those resulting from the use of humanised lymph.

"As regards the number of vesicles, both kinds of lymph have given almost identical results."¹

Opinions which have been expressed in favour of Animal Vaccination.—Various British vaccinators of extensive experience have lately testified to the advantages of animal vaccination. Dr. Ballard, at the meeting of the British Medical Association, held at Leeds in 1869, said that, "two years ago he took considerable trouble to bring this subject of animal vaccination under the notice of the profession of this country. He had been very well satisfied with what he had seen as to animal vaccination at Paris, and subsequently in this country. There were three points in which he thought animal vaccination had a great advantage over arm-to-arm vaccination. One thing was the purity of it, the next was the large quantity that was available, and the third was its effect in the production of very fine vesicles. He must admit the degeneration of the present vaccine virus, as it had been propagated from arm to arm since Jenner's time. He considered that the characters of the vesicle had altered, that the vesicle produced now from the common vaccine, as a general rule, was not so fine nor so certain in its protection—certainly not so certain when used for revaccination—as it was many years ago, and as it was when lymph of recent propagation from the animal was employed."²

Dr. Druitt, at the same meeting, remarked that "he thought medical men should have the opportunity of practising animal vacci-

¹ Op. cit.

² Ballard, 'British Medical Journal,' Sept. 4, 1869, p. 276.

nation, even on the ground that it could do no harm. In the case of a sparse population (he said), where people from their avocations sometimes, as well as their small number, were hardly able to keep up a succession of lymph by means of their children, what could be easier than to get them to arrange to bring their children three or four days in the course of a year, when a calf could be obtained, and when the practitioner and the calf, he would say, must travel together, and in one hour vaccinate the children of the whole population.”¹

Mr. R. Ceely, of Aylesbury, who has examined the subject of vaccination in all its bearings more perfectly than any one living, in a letter to Dr. Blanc, says:—“I hope to be able to find a fair supply of subjects to carry on your lymph, *than which none can be better*. In my last I ought to have said that your primary lymph has all the qualities of that of the spontaneous disease, *except its too frequent acrimony*.”²

Dr. Blanc, speaking from practical experience, thus expresses his opinion:—“Animal vaccination offers the following advantages:—1st. The healthy heifer, inoculated with pure, spontaneous cow-pox, supplies a vaccine lymph free from all morbid and diathetic principles. 2nd. Spontaneous cow-pox, by being transmitted only through the bovine race, retains all its essential qualities. 3rd. Vaccination direct from the heifer offers all the characteristics of the cow-pox, as described by Jenner, Ceely, Bousquet, &c., with such modifications only as are due to the passage of the lymph through young and healthy animals. More activity, later development, and a well-marked deep cicatrix—such are the characters of the vaccination due to heifer’s lymph. 4th. By animal vaccination, we have always on hand an unlimited supply of good vaccine lymph. Vaccination *direct* from the heifer *is always perfectly successful*, and the results I have myself obtained are in every respect excellent. Animal vaccination will in England, as it has already elsewhere, silence all honest opposition.”³

Vaccinators on the Continent, who have tested animal vaccination, testify also in favour of this process. The ‘Report’ of the Committee of the Parisian Academy of Medicine says:—“What we have done proves to the Academy that, at a moderate expense, an establishment for animal vaccination could be organised and maintained, especially in the large towns. The number of punctures which can be made is illimitable, and the quantity of cow-pox which each heifer might furnish is considerable, and in each instance is more than sufficient to meet the exigencies of the most extensive practice. With heifer lymph it would be possible during an epidemic, attacking simultaneously a large number of communes more

¹ Druitt, *Ibid.*, p. 277.

² Ceely, ‘Compulsory Vaccination by Dr. Blanc,’ p. 27.

³ Blanc, *Ibid.*, pp. 22, 23, and 27.

or less distant from one another, to send into the infected localities one or more inoculated heifers, which would furnish all the cow-pox necessary to make vaccinations and revaccinations on a large scale.”¹ The extracts from this report, already quoted, testify also to the very favorable opinion of animal vaccination entertained by this committee.

In a pamphlet on this subject, published recently by Dr. Depaul, he expresses his adherence to the views mentioned in the report of the committee, and to which he has added a further experience of three years. Among other conclusions which he states in this brochure, he says, “Cow-pox practised on the bovine species, cultivated (in other words) on its native soil, preserves during numerous generations an energy and activity which are indispensable for securing its protective properties when inoculated into the human subject. Cow-pox lymph thus perpetuated is a sure means of preventing vaccinal syphilis, and of giving vaccination all the prestige it requires for rendering it truly useful.”²

It is unnecessary, I think, to quote from other writers on animal vaccination—opinions which agree entirely with those we have presented, and vary only in the mode of their expression. Those interested in this question will find valuable material in the pamphlets of Dr. Warlomont of Brussels, of Dr. Fröbelius of St. Petersburg, of Dr. Pissin of Berlin, and of Dr. Lanoix of Paris.³

On the views expressed against the employment of humanised lymph.—The objections raised to the employment of ordinary lymph are—that it is not “pure,” that it has degenerated; and, secondly, that it has been and may be the means of conveying constitutional diseases. Many experienced vaccinators, while discarding the latter objection, have expressed in more or less positive terms their belief in the altered nature of the vaccine lymph at present in circulation.

Mr. Marson,⁴ Dr. Depaul,⁵ Dr. Ballard,⁶ and almost all the authors already quoted, concur in the opinion that ordinary lymph is not identical in its constitution with that derived from a case of spontaneous cow-pox, that it is altered, that it has degenerated.

If ordinary lymph were as effective as the product of natural cow-pox, and if it were equally protective, how are we to explain the fact

¹ ‘Expériences faites à l’Académie Impériale de Médecine,’ &c., p. 54.

² Depaul, ‘Sur la Vaccination Animale,’ 1869, p. 96.

³ Warlomont, ‘De la Vaccination Animale et de l’utilité des Vaccinations,’ 1865.

„ ‘Nouvelle Communication sur la Vaccination Animale,’ 1865.

„ ‘Discours sur la Syphilis Vaccinale et la Vaccination Animale,’ 1866.

Fröbelius, ‘Eine geschichtliche Notiz über die Vaccination im St. Petersburger Findelhause,’ 1869.

Pissin, ‘Reform der Schutz-pocken Impfung durch die Vaccination direct von Kühen in ihrer praktischen Bedeutung,’ 1868.

Lanoix, ‘Étude sur la Vaccination Animale,’ 1866.

⁴ Marson, in ‘Reynolds’ System of Medicine,’ Art. “Smallpox,” vol. i, p. 475.

⁵ Depaul ‘Sur la Vaccination Animale,’ p. 96, 1869.

⁶ Ballard, Ibid.

that the vaccinia developed by it pursues a more rapid course, produces very commonly less perfect typical vesicles, presents on the eighth day more inflammatory action, leaves often very imperfect cicatrices, and is followed in not a few instances by ulceration and loss of tissue at the points of vaccination; while, on the other hand, heifer's lymph develops beautifully typical vesicles, is followed by foveated, well-marked cicatrices, and has never yet been known to be attended by any accident? The cutaneous eruptions which are met with after vaccination with ordinary lymph, the deep ulcerations, and the other "evils of vaccination" (all of which are not infrequently seen), appear to me to distinguish the *irritative* qualities of ordinary lymph from the *infective* qualities of heifer lymph. The latter is perfectly protective, and is not attended with the same untoward results.

The success of revaccination has been repeatedly produced in order to prove the deteriorated character of ordinary lymph. The superior success of revaccination with heifer's lymph over that with humanised lymph shows that the latter is not so protective as it once was, is weaker than it was, and is, in this respect also, inferior to heifer lymph.

All modern experience, I think, confirms the opinion expressed by the immortal Jenner in 1816, that vaccine lymph does not undergo deterioration by mere successive transmission (the subjects for transferring it having been properly chosen); but that it may become altered by transmission through unselected subjects. "The matter," he said, "may undergo a change that may render it unfit for further use by passing even from one individual to another, and this was as likely to happen in the first year of vaccination as in the twentieth."¹ All acknowledge that vaccination has been hitherto, to a great extent, carelessly performed in this country. In Belgium and in Russia this was observed many years ago, and the results there experienced in consequence were an increase of post-vaccinal erysipelas and the occurrence of indolent ulcers after vaccination. I have met with grey-sloughing, eroding ulcers, after apparently successful vaccination with ordinary lymph in healthy children; and, from all accounts, this sequence of vaccination appears to be on the increase. That variola attacks not infrequently young children who have been vaccinated in infancy favours the belief in the diminished efficacy of ordinary lymph.

Lastly, as regards the alleged transmission of diathetic diseases by means of vaccine lymph, I believe there is no proof of such transmission. The far-famed cases of vaccino-syphilitic inoculation, which are said to have occurred in Rivalta, Lupara, and elsewhere, do not stand the test of a searching logical or scientific investigation. The

¹ 'Baron's Life of Jenner,' vol. ii, p. 398.

experiments which have from time to time been made to induce constitutional diseases by injecting morbid secretions into the system, render without foundation the supposition of the communication of blood diseases by vaccination. It is conceivable that vaccine lymph may become so altered by its passage through the human economy—by its forming a part of the individual's life-current while in the system, as to be capable of producing other effects in the vaccinated than those of vaccinia alone. It seems probable that vaccine lymph, if transmitted through unhealthy or poorly nourished children, is impaired in its influence by such transmission. It is reasonable to consider such lymph to have thus been rendered irritative and liable to excite a more or less unhealthy action, though in so mild a degree as to allow the vaccinia to pursue its normal course. To say that vaccine lymph has been removed from vesicles at too late a date, and thereafter been used for vaccinating; that it has been employed on subjects in very indifferent health, and by transmission through healthy constitutions, has again been rendered "pure." Surely this is to treat vaccinia differently from other diseases, and to throw aside the teachings of physiological science and safe logic.

In *conclusion*, it is not necessary to advocate the substitution of "animal vaccination" for "arm-to-arm" vaccination. These two modes of vaccinating are not diametrically opposed; they are complements to each other, and can in this country, as in Italy and elsewhere, be prosecuted simultaneously. It would be most advantageous to establish three or four stations throughout the United Kingdom for the prosecution of animal vaccination. Thither those might repair who desire to have their children vaccinated with heifer lymph, and thence heifer lymph might be procured by those who wish to use it. It would not follow that the adoption of such a plan was an acknowledgment of the deterioration of vaccine lymph by its continuous transmission through the human subject, nor would it be a recognition of the possible conveyance of diathetic disease by means of vaccine lymph; but it would certify that, owing to the hitherto careless performance of vaccination, it is time to derive vaccine lymph once more from its original source; it would be regarded by the public as the earnest of a desire to supply pure vaccine lymph, as the surest means of affording sufficient protection against small-pox; and it would tend to allay fears, remove prejudices, and silence opposition. The Vaccination Act of 1867 is the most perfect legal enactment of the kind which has ever been passed by any government. When thoroughly carried out it will be followed by the best results, and it will as nearly as possible banish small-pox from amongst us. The introduction into this country of "animal vaccination" will prove a most useful adjuvant to the "arm-to-arm" process, and I am persuaded will tend in no small degree to the restoration of public confidence in Jenner's invaluable discovery.

Chronicle of Medical Science.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By ROBERT HUNTER SEMPLE, M.D.,

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On Chloral and its Therapeutical Uses.—The *Bulletin Général de Thérapeutique* expresses its opinion of the value of chloral as a medicine, and summarises the opinions already given as to its efficacy. The substance in question was discovered in 1832 by Liebig, was afterwards studied by Dumas, and has been lately more particularly examined by Liebreich in its physiological relations. Liebreich sets out with the principle that chloral is to be considered as *trichloric aldehyde*, which may be dissolved in an alkaline medium and is then decomposed to form chloroform. The anonymous writer in the *Bulletin* makes use of the *hydrate of chloral* as the most convenient preparation, and he has found, in his experiments on the lower animals, that its effects are quite analogous to those of chloroform. In the human subject, he tried it first in the form of subcutaneous injection, and both he and Liebreich found that the effects were constant and were accompanied by no inconvenience. Various other practitioners have made experiments with this substance, but the results are somewhat contradictory; and M. Bouchut has proved that the different opinions held by the experimenters are due to the fact that impure chloral has sometimes been used. With hydrate of chloral, the results are rapid, evident, and well marked, and these are, the most tranquil somnolence, and insensibility almost absolute. Chloral ought never to be employed in the liquid form, but as a solid hydrate which may be afterwards dissolved in some suitable vehicle. The test of its purity is that it slightly colours a solution of potash with a clear yellow, and disengages a well marked smell of chloroform; if it strikes a brown colour it is impure, especially if the chloroform vapours are mixed with those of chloro-acetic acid. The dose of chloral, given internally, is 2 to 3 or 4 grammes (a gramme is about 15 grains) for a child, and 4, 5, or 6 grammes for an adult, but this dose must not be exceeded. When a patient has been awoke from its effects, the chloral may be repeated so as to induce sleep again, in order that the state of somnolence may be prolonged, if necessary, in painful complaints, as neuralgia, cancer, and burns. A patient may thus take several times 3 to 5 grammes in twenty-four hours, producing each time a period of three to four hours' sleep, but never more than 3 to 5 grammes should be given at a time. The writer in the *Bulletin*

thinks that chloral will occupy a distinguished place in medical practice, as a hypnotic in painful diseases, and as an anæsthetic in a number of small but painful operations in which chloroform is unnecessary. Such diseases as nephritic colic and chorea are adapted for the use of chloral, and the extraction of teeth is rendered painless by its use. It is also useful in tetanus, in extensive burns, and in the paroxysms of gout, and it might be available as an anæsthetic in pregnancy. The conclusions drawn as to the therapeutical applications of chloral are the following. Hydrate of chloral is a powerful sedative of the nervous system, both motor and sensitive, but it must be employed pure. The dose should never exceed 5 or 6 grammes at once in an adult, and the preparations should be used fresh, as they are liable to be altered by keeping. The best mode of administration is by the mouth or by injection, but the use of chloral is contra-indicated in cases of organic disease of the brain and the heart. The effects of chloral are due to the production of chloroform in the system by the chemical action of the alkalies of the blood. It is dangerous, in man, to administer chloral in subcutaneous injections. The hydrate of chloral seldom acts as an emetic and never purges. The temperature is a little lowered by non-poisonous doses of hydrate of chloral, so that it is an algide medicine, and the perspiration is diminished. The hydrate of chloral may be given in definite doses which are calculated to produce anæsthesia, which is not the case with the vapours of chloroform. The action of chloral is exactly that of chloroform, althongh it takes a longer time in producing its effects, but they are more lasting. In some patients the effects resemble those of alcoholic intoxication, but without the disagreeable accompaniments of the latter condition. In almost all persons there is sleep, and the anæsthesia is proportional to the dose employed. Chloral, unlike opium, does not cause vomiting, and does not constipate, but it promotes the appetite, and when the patient is awake, there is neither heaviness nor somnolence. It acts as a sedative in nervous pain, as in the instances above alluded to, and from its action as a sedative in muscular action it is the most prompt and efficacious remedy in intense chorea.—*Bulletin Général de Thérapeutique*, Nov. 30, 1869.

On the Alkaloids of Veratrum Viride and their Therapeutical Effects. By Dr. HORATIO WOOD, jun., of Philadelphia.—Mr. Bullock, of Philadelphia, has described two alkaloids in the *Veratrum viride*, one soluble, and the other insoluble, in ether; and Dr. Wood has investigated the physiological action of these two alkaloids, as well as that of the resin associated with them. His experiments were made upon some of the lower animals with the alkaloids in question, to which it is proposed, in the forthcoming edition of the *United States' Dispensatory* to give the names *viridia* and *veratroidia*. The general results at which Dr. Wood has arrived are that there exist considerable differences in the action of *viridia* and *veratroidia*, the former being only slightly, if at all, locally irritant, never producing vomiting or purging, exerting no direct influence on the brain, but acting as a depressant of the spinal cord and of the circulation, while the latter (*veratroidia*) is somewhat irritant when locally

applied, and is an irritant emetic, and sometimes a cathartic. But, like viridia, it exerts no direct influence on the brain, and is a depressant of the spinal cord and of the circulation. Veratroidia appears to hold, physiologically, as well as chemically, a middle position between viridia and veratria. The resin of the veratrum viride appears to have no very active properties. Dr. Wood thinks that viridia will prove to be a valuable remedy, possessing all the sedative influence of the veratrum viride without the nausea and vomiting which the plant itself often produces. Dr. Wood has taken viridia himself in order to test its properties, and he thinks that this alkaloid might be given in the dose of one sixth of a grain repeated every hour.—*American Journal of Medical Sciences*, Jan., 1870.

On the Effects of Arsenic in Phthisis. By Dr. ISNARD, of Marseilles.—The effects of arsenic in the treatment of phthisis have already been investigated by Dr. Cersoy, of Langres, and Dr. Isnard has lately contributed some of his experience on this subject in memoirs published in recent years. Dr. Isnard now gives a summary of his views in reference to the local and general action of the drug. He states, in the first place, that when arsenic is employed in phthisis, the febrile disturbance, when it exists, is weakened and suspended, while the nocturnal sweats, the general excitement, and the sleeplessness are also diminished. As the fever abates the digestive function is improved, and the diarrhoea, or constipation, or vomiting disappears; in short, a general improvement becomes perceptible. As the constitution improves, the local lesions and the lung itself undergo a beneficial change, and the cavities in the lung are cicatrised. This result is proved, according to Dr. Isnard, by the relief of the cough, the diminution of the secretion of the bronchial tubes and of the pyogenic membrane of the cavities, by the substitution of mucous for purulent sputa, and of dry for humid rhonchi. Another and no less important result of the arsenical treatment is the retardation of the evolution of tubercles which are not yet softened, and the arrest of development of those which were about to form. Many tubercles are thus rendered abortive, or they remain in a latent condition, or they do not pass beyond their period of crudity. Dr. Isnard then examines more particularly the immediate action of arsenical preparations upon the respiratory function, the respiratory organs, and the tubercles. Arsenic promotes respiration, as is proved by its physiological and therapeutical effects, and Dr. Isnard states that arsenic eaters, and those who take arsenic for other diseases besides those of the lungs, are relieved of the dyspnoea caused by various pulmonary diseases. The drug also acts beneficially on the pulmonary tissues and the nerves and muscles of respiration; but Dr. Isnard's views on this point are somewhat hypothetical, as are also those which relate to the action of arsenic on the tubercle itself. He considers that the arsenic, by its regenerative action on our tissues and functions, is well adapted to remedy the organic disturbance which engenders phthisis, and that it impresses on the economy a vitality which is superior to, and incompa-

tible with, the development of tubercle, and thus renders the soil unfit for its reproduction. The general conclusion drawn by Dr. Isnard as to the action of arsenic in phthisis is, that by its local and general action, at once curative and preventive, it influences at once the capillary system and the different tissues, affecting both the lungs and the whole economy. It does not attack the tubercle directly and specifically like a parasiticide, but directs its action to the elements and tissues which remain actually or relatively healthy. —*Bulletin Général de Thérapeutique*, Dec. 30, 1869.

On the Treatment of Asthma by Tobacco. By Dr. SÉE.—The effects of tobacco are primarily those of a poison, but they differ from those of other poisons in being modified by habit. In this respect tobacco agrees with opium, belladonna, haschish, and arsenic, although all of these are tolerated less than tobacco, while, on the other hand, some medicines accumulate in the system, such as digitalis and strychnia. Dr. Sée considers that tobacco, used in moderation, accelerates the respiration in the first instance, but in larger doses that it causes tetaniform convulsions of the respiratory muscles, and he therefore advises asthmatic patients to use tobacco cautiously, for when too much is employed the very symptoms are produced which the sufferer wishes to avoid. His opinion is that tobacco, like belladonna, acts upon the origin of the pneumogastric nerve without affecting the nerve itself: and that its first action, used in a moderate dose, is to accelerate the breathing, while its secondary action, and in an increased dose, is to retard it. Thus, he continues, tobacco and belladonna act upon the same central organ as asthma does, and he argues, moreover, that this action is inversely to that of asthma, so that the association of the two medicines in moderate doses at the commencement of the asthmatic paroxysm is legitimate and useful. In giving advice, therefore, as to the use of tobacco by asthmatic patients, he says that they should be recommended to consume only a certain dose, for if tolerance be exceeded, then the circulation and the respiration will be retarded, the vessels will be dilated, and all these unfavorable effects will promote the return of the paroxysms.—*Ibid.*, Nov. 15, 1869.

On Cincho-Quinine, a new preparation from Peruvian Bark—In a paper published in the 'Boston Journal of Chemistry,' it is proposed to employ in medicine a preparation containing all the alkaloids of the cinchona barks, instead of the barks themselves, or any one of the alkaloids separately. Cincho-quinine contains quinia, cinchonina, quinidia, cinchonidia, and other alkaloidal principles which have not been distinctly isolated, and the precise nature of which is not well understood. In the paper referred to, cincho-quinine is said to be preferable to sulphate of quinine, inasmuch as it does not produce cerebral distress as the latter sometimes does, and it does not oppress the stomach or create nausea. It has also the great advantage of being nearly tasteless, the bitter being very slight, and it is less costly than the sulphate of quinine. It forms a perfectly clear solution in water acidulated with acetic or sulphuric acid, and a pleasant

elixir may be made from this solution by adding syrup and aromatics. In intermittents, cincho-quinine may be given in the same doses as sulphate of quinine, but it is recommended that a dose of ipecacuan or other emetic should be given as an introduction to the treatment of fever and ague. This mode of treatment is said to be very successfully carried out in the wards of the United States Marine Hospital, Chelsea.—*Boston Journal of Chemistry*, 1869.

On the Therapeutical Action of Sambucus Canadensis in Albuminuria. By Dr. ROBERT MACNUTT, United States.—Dr. MacNutt describes two cases of albuminuria in which a cure appears to have been effected by the administration of *sambucus canadensis*, or the inner bark of the Canadian elder tree, steeped in cider. One case was that of a lady, aged 75, suffering from renal dropsy which had resisted all ordinary treatment for many months, and was considered incurable. Some of the neighbours of the patient had heard of dropsy being cured by the elder bark, and accordingly a quantity of this bark was put into a large bottle, hard cider was poured over it, and it was taken three or four times a day, the dose of the preparation being an ounce. In a few days improvement commenced and eventually a cure was effected. The other case was that of a little girl, aged 7, suffering from dropsy with albuminous urine, and after all the usual remedies had been employed without benefit, the elder bark effected a complete cure. Dr. MacNutt considers the elder bark as almost a specific in albuminuria, and he believes that it corrects the morbid condition which results in the elimination of albumen by the kidneys. The editor of the journal in which the paper appears endorses this opinion, and states moreover that the use of elder bark has long been recommended in dropsy.—*American Journal of the Medical Sciences*, July, 1869.

On the Subcutaneous Application of Veratria. By Dr. PÉGAITAZ, of Freiburg.—Dr. Pégaïtaz, in a very long, learned, and elaborate paper, relates his own researches on himself, on patients, and on the lower animals, with subcutaneous injections of veratria, and he prefaces his practical researches with a detailed history of the use of the alkaloid as recorded by various writers who have employed it in the treatment of disease. The substance called at present veratria, was discovered in 1818 by Meissner, and was called by him sabadillin, but it was also obtained in the next year by Pelletier and Caventou and called by them veratria. Meissner obtained it from the seeds of *Veratrum sabadilla* (now called *Asagrea officinalis*) and Pelletier and Caventou from the root of the *Veratrum album*. The first physiological experiments on this substance were made by Majendie and Andral in 1820, and after its effects had been observed on the lower animals, it was introduced into medical practice, though at first it was used only as an external application. Its internal use was recommended by Majendie, and Turnbull, in 1834, published the favorable results which he had obtained from its use in some nervous complaints. It was also used internally, and with considerable success, in France by Aran, Trousseau,

and Bouchut, in several inflammatory diseases, especially acute rheumatism and pneumonia, and in Germany, Dr. Kocher published a work containing his experience of the use of the alkaloid. In reference to the hypodermic use of veratria, which is the main subject of Dr. Pégaitaz's paper, the author first relates his experiments on dogs and cats, the results being carefully recorded in each case. The principal phenomena observed were, first excitement, and then depression, afterwards salivation, nausea, vomiting, generally with diarrhoea, unsteadiness in the movements of the extremities, with stiffness in these parts, elevation of the reflex irritability and diminution of sensation, transient lowering of the temperature, of the respiration, and of the frequency of the pulse, and also convulsions and tetanus. These results are essentially the same as those observed after the internal administration of the alkaloid. The records of the therapeutical applications of subcutaneous injections are very scanty, and are not satisfactory, but Dr. Pégaitaz gives the results of his own experiments on patients. He treated three cases of pneumonia, and one of pleurisy, the preparation used being tartrate of veratria in an alcoholic solution, and the phenomena are recorded with very great care, the observations being taken every hour, and the temperature, the pulse, and the respirations being noted, together with all the other conditions of the patient. In all the cases there was very great pain in the place of injection immediately after the operation, and Dr. Pégaitaz himself also testifies to the great pain produced, stating that in his own case a neuralgia from which he suffered was more endurable than the pain of the injection. The conclusions at which Dr. Pégaitaz arrives, after his laborious researches, are on the whole unsatisfactory, because the pain of the subcutaneous injection contraindicates its adoption. In other respects he finds that the subcutaneous injection of veratria acts more rapidly and intensely than the internal use of the alkaloid, that the accompanying phenomena are the same in both cases, and that the results produced are the same in man as in the lower animals.—*Deutsches Archiv für Klinische Medicin*, September, 1869.

On the Nitrogenous Tepid Water of Buxton. By Dr. W. H. ROBERTSON.—The tepid nitrogenous water of Buxton is chiefly celebrated for the relief it affords in cases of rheumatism and gout, and other allied affections. The temperature of the water is uniformly 82° Fahrenheit, and Dr. S. Muspratt has estimated the amount of nitrogen as 504 cubic inches to the gallon. The water is used as a bath, and it is also taken internally, but the bath is said to be the preferable mode of employment. The water is said to be primarily a stimulant, and secondarily an alterative. The younger the patient, the more recent the case, and the nearer to the surface the part that is affected, the more rapid and lasting is the cure; but in cases of hereditary predisposition the result is less satisfactory, and the cure more tedious and difficult. The use of the bath at the natural temperature is more efficacious than when it is heated to a higher degree,

and indeed its curative power seems to be lessened in proportion to the elevation of the temperature. The effect of the bath is said by Dr. Robertson to depend on the amount of friction of the surface used during the immersion, and on the absorption of the gas through the skin. The Devonshire Hospital and Buxton Bath Charity is said to have received 49,236 patients in about forty-eight years, and about 42,200 have been cases of rheumatism. Of the total cases, 44,744 are said to have been discharged more or less cured or relieved. Buxton itself is essentially an inland climate, being situated almost at an equal distance from the eastern and western coasts; it is one thousand feet above the level of the sea, and is upon the mountain limestone formation, but close to its edge.—*British Medical Journal*, Oct. 16, 1869.

On the Employment of the Oil of Turpentine in the Tympanites and the Nervous Symptoms of Dothinentérie (Typhoid Fever). By Dr. CANTEL.—Although the oil of turpentine has been very strongly recommended by English medical writers in many cases of fever and nervous disease, the use of this remedy has not been hitherto very much appreciated in France, where it has been employed successfully only in such cases as sciatic neuralgia and some kinds of colic. But Dr. Cantel, following the precepts of Dr. Graves, of Dublin, has found it very serviceable in tympanites and the nervous symptoms which constitute the dangers of adynamic fever. He relates a case in which the use of the oil of turpentine appears to have been efficacious, and from the success which he attained he was led to employ it afterwards in similar cases. Dr. Cantel does not regard the oil of turpentine as an infallible specific in dothinentery of the abdominal and ataxic form, but he has always observed a temporary relief by its diminishing meteorism and promoting the relaxation of the bowels. He has never found it to aggravate the local or general symptoms of the disease, but he refrains from administering it when there is inflammation of the intestine. He employs the turpentine in the form of frictions, consisting of turpentine and olive oil, when the least meteorism is observed; and if the inflation of the abdomen is more marked, he gives, night and morning, an injection by the rectum of decoction of camomile, to which are added a few spoonfuls of the turpentine liniment; and if the distension of the abdomen is very considerable, he administers the mixture recommended by Graves, consisting of turpentine, castor oil, and water, given in teaspoonful doses every hour. (It should be noticed that French writers do not regard typhoid fever as the same disease described under that name in modern English medicine.—REPORTER.)—*Bulletin Général de Thérapeutique*, July 30, 1869.

On the Treatment of Epilepsy by Bromide of Potassium. By Dr. PAUL-MAX SIMON.—Dr. Simon communicates the particulars of ten cases of epileptic patients treated by bromide of potassium, and he offers some remarks upon the effects produced on the malady by the use of this salt. The results were not by any means satisfactory in all the cases. Dr. Simon considers, for instance, that the bromide is

seldom beneficial in old cases of epilepsy. In five of the cases the use of the bromide caused a suspension of the attacks for shorter or longer periods, but they returned at length in a more violent form than ever. The same result has been previously observed from the treatment of epilepsy by belladonna, quinine, valerianate of zinc, the salts of copper, &c., for all these things have been said to cause a longer interval than usual between the attacks, which, however, at length broke out more violently than before. If however, Dr. Simon argues, the morbid tendency could be averted by warding off the attacks, then those remedies, including chloroform, which arrest the paroxysms might be considered very valuable; but if the result is only to redouble the violence of the paroxysms at last, the real efficacy of such medication may be doubted. Still Dr. Simon does not deny that bromide of potassium is useful in epilepsy, and two of his cases recorded in the paper confirm that view. Among the conclusions he draws as the result of his observations, he finds that by the use of the bromide, in the greater number of cases, the maniacal delirium is increased, and he states that it was in a case of deep torpidity of mind induced by numerous epileptic attacks, that he found the use of the bromide most signally efficacious.—*Bulletin Général de Thérapeutique*, Dec. 15, 1869.

On the Therapeutical Use of Bromide of Potassium in the Complaints of Early Childhood. By Dr. MOUTARD MARTIN, of the Hôpital Beaujon.—Dr. Moutard Martin, observing that the bromide of potassium acts in tranquillizing the nervous system, and that it may be given in large doses without any bad effect, thought that the administration of the salt might be useful in some of the complaints of early childhood, and he accordingly instituted a series of inquiries upon the subject, and presented the result to the *Académie Impériale de Médecine* in Paris. He has employed the bromide chiefly in those cases where the children, without having any special disease, do not sleep, and by their restlessness prevent their nurses or other attendants from sleeping. But sometimes the restlessness is caused by teething, and then, if there be no serious malady induced, such as fever or obstinate diarrhœa, Dr. Moutard Martin has found the bromide act favorably. He is led to inquire whether its operation is due to its general sedative influence over the nervous system, or to the anæsthetic effect which it is known to exert over the mucous membrane covering the gums and other parts in the inside of the mouth, and he inclines to the latter view. From the result of all his researches Dr. Moutard Martin concludes that the bromide, administered in moderate doses, is perfectly well borne by children of tender age; that by its sedative action it cures the insomnia of infancy; that when administered to children who present any of the unfavorable symptoms of the period of dentition, such as excitement, sleeplessness and cough, it often affords relief, or may even sometimes prevent convulsions: that it should not be given where there is diarrhœa; and that in certain cases where there is great excitement of the nervous system, it sometimes produces marked and immediate benefit.—*Bulletin Général de Thérapeutique*, Nov. 15, 1869.

REPORT ON PATHOLOGY AND PRINCIPLES AND
PRACTICE OF MEDICINE.

By FRANCIS C. WEBB, M.D., F.L.S.,

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The Pathology of Cerebral Hæmorrhage.—Charcot and Bouchard, examining the causes to which cerebral hæmorrhages are usually referred, find that they may be arranged in three groups:—(1) Diminution of the consistence of cerebral tissue to such a degree that it does not furnish sufficient support to the vessels; (2) Increased tension of the blood, depending on hypertrophy of the left ventricle, atrophy of the kidneys, &c.; (3) Diminished resistance of the vessel in consequence of change in the walls (fatty degeneration or atheromatous incrustation).

Some of these (as hæmorrhagic ramollissement, for example,) are doubtful; others appear to be only accessory. Thus the authors find, with regard to arterial atheroma, that it is not present in 22 per cent. of cases, and that it is only present in a marked degree in 25 per cent. Again, hypertrophy of the heart was not present in 40 per cent. of cases. These statistics are founded on the examination of sixty-nine cases. The only pathological condition which MM. Charcot and Bouchard have constantly met is an aneurismal state of a certain number of the small intra-cerebral vessels. From the small size of these aneurisms the authors give them the name of miliary aneurisms. They are visible to the naked eye. They appear as small globular grains, varying in diameter from two tenths of a millimètre to a millimètre, and sometimes a little larger. If they contain liquid blood, their colour is red or violet; if, on the other hand, the blood is con-creted or has undergone transformation, the aneurism is red-brown, ochry, or even blackish. The colour is also influenced by the variable thickness of the wall. The optic thalami, the corpora striata, the convolutions, the pons, the cerebellum, the centrum ovale, the middle peduncles of the cerebellum, the cerebral peduncles, the medulla oblongata, are the parts in order of frequency where these aneurismal dilatations have been most frequently met by the authors.

This distribution, they remark, agrees with that of cerebral hæmorrhages. Seen under the microscope with a low power, the vessels present a fusiform or sacciform dilatation. With a higher power it is seen that the wall of the aneurism is continuous with the tunics of the vessel which is its seat, but the three tunics are not distinct, they are fused, and the thickness of the aneurismal wall is less than that of the three normal coats of the vessel, a fact which accounts for their proneness to rupture. Careful observation of the aneurismal vessel reveals alteration of structure (arterite sclereuse). This change proceeds from without inwards, for the most considerable changes have their seat in the outer parts of the vessel, and the atrophy of the

muscular coat depends on change in the adventitious tunic—whence the name periarteritis which they propose for it.—*Archives de Physiologie Normale et Pathologique*, and *Gaz. Méd. de Paris*, Mars 12, 1870.

The Microscopic Morbid Appearances met with in the Brains of the Insane. Doctors J. B. Tuke and William Rutherford have published some careful observations of the morbid appearances presented by the brains of thirty insane persons. All the brains examined were those of patients who had been the subjects of chronic insanity. The forms of disease under which they had laboured were general paralysis, dementia with paralysis, chronic dementia, epileptic insanity. The autopsies were made within thirty-six hours after death. The specimens of nervous matter were immersed in spirit and afterwards in a solution of chromic acid. The sections were coloured with ammoniacal solution of carmine, rendered transparent by spirit of turpentine, and set up in Canada balsam or glycerine. The parts of the brain examined were the tips of the frontal and occipital lobes, the convolutions on either side of the fissure of Rolando near the vertex, corpora striata, optic thalami, cerebellum, pons varolii, medulla oblongata, and any portion which was manifestly the seat of disease. In every case marked departure from healthy structure was observed. The morbid appearances noted were as follows:—1. Grey degeneration, or sclerosis. This lesion has been observed by the authors (1) in the white matter of the corpus striatum; (2) in that of the cerebrum and cerebellum; (3) in that of the pons and medulla oblongata. It was found in eighteen instances; in nine of chronic dementia; in three of chronic mania; in three of general paralysis, and in three of epileptic insanity. This lesion the authors have observed in the white matter only. They agree with Rokitansky in regarding it as primarily a modification of the connective tissue or neuroglia, viz., fibrillation and increase of the neuroglial matrix, together with proliferation of its nuclei. The nerve tubes in the diseased tracts *appear* to undergo atrophy. The authors have seen no evidence of the production of fatty particles from the wasted tubes in the brain. Colloid bodies sometimes appear in degenerated tracts, but not frequently. 2. Miliary sclerosis. Semi-opaque whitish spots, resembling very small millet seeds; they vary in size from one-fortieth of an inch downwards, and are almost confined to white nerve-matter. They consist of semi-opaque molecular material lying amid a few exceedingly delicate colourless fibres. This lesion is due to the formation of cell-like masses of molecular material, in each of which a nucleus is sometimes discernible. By these bodies the nerve-tubes and blood-vessels are pushed aside, and may be seen curving round the diseased tracts. The authors suppose that these bodies originate in the nuclei of the neuroglia, not by proliferation, but by the transformation of a single nucleus into a cell-like body. This change they have observed in the white matter of the convolutions in four cases, in one in the white matter of the cerebellum. 3. Holes, in the white matter of the brain, varying from one-fiftieth of an inch downwards. The authors have met them most frequently in the corpus striatum, but also in the pons Varolii, and in the white matter of the convolutions. The holes have ragged edges

owing to the abrupt termination of nerve elements at their margins. The authors are uncertain as to their nature. Unlike those recently described by Lockhart Clarke, they have no apparent connection with blood-vessels. 4. Changes in nerve-cells. Atrophy of the pyramidal cells of the convolutions of the vertex was observed in three cases. They had shrunk to one eighth of their normal size. Pigmentary degeneration of nerve-cells was frequently observed in the pyramidal and other cells of the cerebral convolutions, in the cells of the corpora striata and the corpora dentata of both cerebellum and medulla oblongata. For this change the authors suggest the name *fuscous degeneration*. 5. Changes in nerve-tubes. Atrophy of nerve-tubes was frequently observed. Pigmentary degeneration of the nerve-tubes of the anterior and posterior roots of the spinal nerves was observed in a case of chronic dementia complicated with chorea. 6. Changes in blood-vessels. Fatty degeneration of the vessels of the pia mater and cerebrum was frequently observed. In general paralysis, tortuosity and aneurismal dilatation of the capillaries; on the walls proliferation of neuroglial cells and deposits of hæmatoidin were not unusual. In a case of paralytic dementia there was marked dilatation of the perivascular canals. 7. Granulations of the lining membranes of the ventricles and of the pia mater of the parietal and occipital lobes, and of the medulla oblongata, were observed in many cases of general paralysis, and old standing mania and dementia. They were the result of changed epithelium. 8. Amyloid and colloid bodies were seldom absent from the brains of old standing cases. The former readily coloured with carmine not exceeding twice the size of the human blood-corpuscle, the latter much larger and unaffected by carmine. The names amyloid and colloid are applied without reference to theory to distinguish them. "Virchow holds that one or other or both of these bodies are normal, or at least the result of natural decay. But the fact that they are more common in the brains of the insane than the sane, and the immense numbers in which they occur, suggests that they are actual morbid products." 9. Changes in nuclei of neuroglia. In some cases the number seemed diminished; proliferation, however, was more common. The authors do not regard the morbid appearances they have enumerated as the cause of insanity. They think it more likely they are the result of weakened or perverted evolution of the nerve-plasm. Nor do they regard these lesions as confined to cases of mental insanity, for they are well aware that certain of these morbid appearances occur in subjects where the insanity has only affected the muscles, and where the mental faculties have remained unimpaired.—*Edinburgh Medical Journal*, October, 1869.

Two Cases of "Porencephalie."—M. Heschel has reported two cases of a rare affection, to which he gives the name "porencephalie." Fistulous passages exist, establishing a communication between the arachnoid cavity on the surface of the hemispheres and the lateral ventricles. The following are the cases:—A man, æt. 42, tuberculous. The left arm one fifth shorter than the right, atrophied. The right inferior extremity is a little thinner than the left. *Autopsy*.—Cranium

hard and thick. The whole dura mater covered by points of exudation. In the right temporal region the dura mater was adherent with the pia mater, and there were scattered adhesions towards the convolution of the hemispheres. From the part of the temporal region where the adhesion existed, a canal traversed the cerebral mass, and opened into the inferior cornu of the right lateral ventricle. The external opening of this canal was oval, five centimètres in length, three in breadth; it was slightly narrowed in the lower part. The walls of the canal were brownish. The rest of the brain was healthy. The lungs and liver were filled with tubercles; the latter contained, also, echinococci.

The second case was observed in 1865, it was that of a man, æt. 55, also tuberculous. *Autopsy*.—The cranium was thick and compact. The brain of normal volume; the left cerebral hemisphere was traversed by a canal, which commenced at the origin of the principal superior convolution by a round opening, an inch wide, and proceeded a little forward to the left lateral ventricle. The external opening was covered by the cerebral membranes, and the walls were membranous. The rest of the brain was healthy. There were exudations on the dura mater, with depression of the surface of the cerebrum. Tubercles of the lungs, caries of the dorsal vertebræ.—*Prag. Vierteljahrschrift für die Praktische Heilkunde*, 1868; *Archives Générales de Médecine*, February, 1869.

Loss of Sensibility to Pain (Analgesie) in Vertebral Caries and Histological Alterations of the Spinal Cord.—Rosenthal reports the case of a woman, æt. 44, who had suffered for two years from pains in the legs and the dorsal region, and from paralysis of the lower limbs. The vertebral column was affected with angular curvature, which extended from the sixth to the ninth dorsal vertebra. Electrical examination showed conservation of electro-muscular contractility and loss of electro-muscular and electro-cutaneous sensibility. Tactile sensibility was normal, but sensibility to pain and to temperature was lost over a large portion of the body. The analgesia occupied the anterior part of the lower limbs, and extended to the fifth rib above, behind to the third lumbar vertebra. The inferior part of the buttocks, the anus, and pudendum, retained sensibility. At the end of a month the analgesia gained the inferior part of the buttocks and the posterior surfaces of the thighs. At the end of three months cramps of the extensors of the paralysed limbs supervened, with paralysis of the sphincters. Death occurred shortly after from prolonged decubitus and anæmia. The autopsy revealed œdema of the brain and a constriction of the cord, with softening between the sixth and eighth thoracic nerves. Microscopical examination made by Dr. Meynert showed at this spot traces of old myelitis occupying chiefly the anterior part of the posterior columns near the posterior cornua. Above and below this nucleus of inflammation, the grey substance, from the upper part of the cervical region as far as the medullary cone, was the seat of grave alterations involving the nerve-cells (sclerosis, and fatty and pigmentary degeneration). This histological demonstration of the lesions of the nerve-cells of the grey substance in analgesia confirms some experiments

of Schiff, who found that if he divided the grey substance in a rabbit, taking care of the posterior columns, the parts behind the section preserved tactile sensibility, but became insensible to pain.—*Société de Médecine de Vienne*. (Reported by Prof. Beaunis.) *Gaz. Méd. de Paris*, Sept. 11, 1869.

Fibro-plastic Tumour of the Base of the Skull, with Leucocythæmia.—L. G—, æt. 22, married. In June, 1867, a small hard tumour was noticed in the right parotid region. This was followed by the appearance of several smaller ones extending along the submaxillary region of the same side. Six months later, a tumour similar to the primary one appeared in the left parotid region, and this was followed by several smaller ones about the parotid and submaxillary regions of the same side. Patches of ulceration then appeared in the skin of the right temporal region; the right eye then became prominent, and continued so for some time, when a small discharge of pus took place from two openings, one just under the supra-orbital ridge, and another just over the inferior border of the orbit, after which the eye resumed its natural position and appearance. About the time of the eye protruding the power of vision and the power of moving the eye on that side were lost, and there was paralysis of the upper lid. There was no anæsthesia of the globe, nor of the integuments, nor any strabismus. Smell was slightly impaired. In the latter part of her illness mild clonic convulsions came on during sleep, and lasted from five to thirty minutes. During the last two or three months she breathed entirely by the mouth, the nasal passages being obstructed. For some time previous to death there was partial closure of the jaws, and difficulty of deglutition, produced by pressure of the enlarged glands. She died February 23rd, 1869. On February 3rd her blood was examined, and showed considerable increase of white corpuscles—there being from twelve to seventeen in a moderate microscopic field. *Sectio cadaveris.*—Mass of glands in right parotid and submaxillary region; an enlarged gland in left parotid region, and several in the left submaxillary region. *Head.*—Several patches of ulceration involving only the skin in the right temporal region. Serum in arachnoidal cavity; brain rather less firm than usual. On removing the brain a morbid growth was found, involving the body of the sphenoid and basilar portions of the sphenoid and occipital bones, and extending into the right middle fossa of the base of the skull. The growth surrounded the second, third, fourth, and fifth nerves of the right side, and the third and fourth of the left side. The bones involved by the disease were partially absorbed. The portions remaining were cut through, and the growth was found to involve the ethmoid, and portions of other surrounding bones, and it partially filled the nasal cavities. Three or four mucous polypi occupied the remaining portion of the nares. The right orbital roof was cut through, and the post-ocular cellular tissue found to be in a healthy condition, there being no evidence of there having been suppuration in the orbital cavity. The morbid growth did not extend into the cavity of the orbit. The dura mater covering the growth appeared healthy. *Thorax.*—Thymus enlarged. Small

cavity in apex of left lung; grey granulations and cheesy mass in right. *Abdomen*.—Intestines normal, except one small glandular-looking patch, situated about five feet above the ileocæcal valve. Supra-renal capsules enlarged; pancreas slightly enlarged. Blood from the large veins showed a relatively large number of white corpuscles; that taken from the splenic vein contained a very large number. The glandular tumours and the morbid growth at the base of the brain were examined by Dr. John Homans, and were found to be composed of small spherical and ovoid cells, containing distinct nuclei, and somewhat granular, together with more or less nucleated fusiform cells; but there were very many less of these latter in the glandular tumours than in the other morbid growth. The author remarks that the functions of the different nerves do not seem to have been impaired strictly in proportion to the apparent extent to which they were respectively involved by the disease.—J. B. Treadwell, M.D., *Boston Medical and Surgical Journal*, April 1st, 1869.

On the Diagnostic Value of the Corpuscular Blood Elements in the Urine of Bright's Disease.—Dr. Joseph G. Richardson bases his researches on Cohnheim's observations on the process of pyogenesis, which he says have been tested and corroborated by J. Woodward, of the Surgeon-General's Office at Washington. Cohnheim's view of pyogenesis is that it "consists, first, in a partial interruption of the flow of blood, by which the red corpuscles move more slowly through or almost block up the capillaries, while white globules, adhering to the parieties of the vessels, arrange themselves in a layer upon the inner surface of the walls; and secondly, in the 'wandering out' of the white blood-cells through the stomata, demonstrated by Recklinghausen in the finer blood-vessels, by virtue of that amœboid movement which is one of the most remarkable attributes of the white blood-corpuscles." Pus consists of the white blood-corpuscles which have thus wandered out and of exuded serum.

Dr. Richardson applies these observations to the elucidation of the pathology of kidney disease and the microscopic appearances presented by the urine. He has been anticipated, however, partly by Professor Axel Key, of Christiania, who, in a paper translated into the '*Medical Times and Gazette*' of May 22nd, 1869, states that, from his experiments made by setting up inflammation of different grades in the kidneys, and at the same time throwing cinnabar injections into the blood, he finds that even in slight irritation the white blood-cells, without interstitial changes, and without hæmorrhage, escape from the vessels into the renal glomeruli, and force their way into the tubes, sometimes one by one, and sometimes several together, and are found in the urine under the appearance of pus cells, or Professor Key believes the leucocytes may remain on the lining membrane of the tubules and develop into epithelium. He further states: "In morbid changes in the kidneys the cells which have migrated from the glomeruli, and from the interstices into the tubes, and which resemble pus-cells, may occur in great abundance in the urine, both isolated and forming whole cylinders, without the slightest trace of suppuration or actual pus formation being discoverable in the kidneys."

Dr. Richardson believes that the simplest form of renal inflammation may be produced by some mere mechanical cause, the pressure, for instance, of a small uric acid calculus or a plug of adherent white blood-corpuscles. This produces a partial or complete stasis of blood in the neighbouring vessels, whose first or least amount results in the migration of a few leucocytes and the exudation of the liquor sanguinis, which, mixing with the urine, renders that fluid slightly albuminous; the second or medium degree results in the formation of tube casts, called fibrinous (sometimes on insufficient evidence), and the abundant wandering out of the white (accompanied by a few red) blood cells, through their power of amœboid movement; lastly, when of its third or highest intensity, in the free escape of the red blood-discs, in addition to the other elements alluded to, through the walls of the minute capillaries. He believes that red and white corpuscles occurring in their natural proportions in the urine indicate renal hæmorrhage; when the two kinds of corpuscles are more nearly equal in number they indicate acute or subacute nephritis; and that the existence of white blood-cells (pus-, mucous-, or exudation-corpuscles) without red generally shows a chronic or, at least, less active inflammatory condition of the kidneys. Further, that careful comparative microscopic examinations, performed at intervals of a few days, afford an important guide to the effect of treatment and the progress of the disease.—*American Journal of the Medical Sciences*, January, 1870.

On Alopecia Areata.—Dr. Thomas Reid, after noticing the vague and conflicting opinions held as to the nature and cause of alopecia areata, describes three cases in which the condition of the roots of the hair was examined microscopically in different stages of the disease. From these he draws the following account of the development of the parasitic fungus *Microsporon andouini*, on which he believes the disease depends, and of the progress of the affection:—"Three stages, at least, can be traced with considerable accuracy. In the first, the parasite having effected a lodgment in the hair follicle, in the form of fine granular matter, diffuses itself among the epithelial scales, producing a certain amount of thickening and enlargement of the sheath. This condition of the sheath, by simple mechanical pressure exerted on the neighbouring parts, probably gives rise to the erythema and neuralgia frequently accompanying this stage. In the second stage the spores of the parasite can be traced as far as the soft bulb, ramifying in the intercellular spaces and interrupting the nutrition of the hair. This interruption of the nutrition, and the irritation caused by the presence of the parasite, induce certain changes in the structure of the hair, which continues to grow. These changes are bulbous or fusiform dilatations of the hair, chiefly in the medullary substance. The hair fibres become opaque and atrophied, sometimes containing fine granules in their interior. The diseased hair still continues to grow, but on reaching the surface of the skin breaks off, being too brittle to sustain its own weight. In the third stage the spores are mixed with filaments, and the parasite has arrived at maturity. The root is completely atrophied, its connection with the papilla is severed,

it dies, and is thrown off as a sequestrum, or is pushed out by the growth of the succeeding downy hair. Most frequently the hair and its sheath are extruded separately—in such cases the parasite can seldom be detected on the body of the root. The sheath (or, at least, the spores of the parasite) may remain, and surround the downy hair on which they have been occasionally detected, and thus a second crop of the disease may be produced.

“It has been stated that the papilla appears to undergo certain changes. We know that in most cases of alopecia its power of developing new hair is very much weakened at first, as is evidenced by the weak downy hairs which generally succeed those which are extracted; but it is quite conceivable that in aggravated forms of the disease the papilla is also destroyed, and permanent alopecia will be the result, a condition sometimes found to exist along with downy patches on the same head” (Dr. T. Read).—*The Glasgow Medical Journal*, May, 1869.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIÈNE.

BY BENJAMIN W. RICHARDSON, M.D., F.R.S.

On the Applicability of the Solution of Saccharine Oxide of Iron as an Antidote for Arsenical Poisoning.—Dr. H. Köhler, of Halle, publishes a valuable and elaborate paper on the saccharine oxide of iron, in solution, as an antidote for poisoning by arsenious acid. In his paper, in order to place the subject in hand so that a correct judgment may be formed respecting the value of the antidote, he follows out three lines of experiment, one chemical, a second physiological (by experiment on animals), and a third practical, viz. by observing the effect of the antidote on the human subject after poisoning by arsenic. Throughout a careful comparison is sustained between the respective value of the solution of the saccharine oxide of iron and the older antidote of hydrated ferric oxide, which forms with the soluble an insoluble arseniate, and has been now for some years employed with marked success. In his chemical researches Köhler observes, amongst other subjects, whether, by the solution of saccharine oxide of iron acidified, the arsenious acid, in a solution, can be perfectly precipitated; whether the precipitation will occur when the mixture is greatly diluted; whether the presence of artificial gastric juice influences the process; and whether the existence of large quantities of albuminous matter in artificial gastric juice hinders or increases the precipitation. In his research on animals he inquires how far the administration of the antidote taken with the poison differs in effect from its administration soon after the poison has been taken by itself; and in his research on the human subject he

discusses if the results obtained on lower animals from the antidote are to be depended upon as reliable. He relates, in full, one case in man in which the antidote was put to the test. The subject was a youth of eighteen, who poisoned himself by taking from two to three grammes of arsenious acid in powder, and suspended in water. After the act the patient quickly repented what he had done, and he was visited within one hour. The treatment was commenced immediately, and consisted of the administration of a large teaspoonful of the saccharine oxide of iron, with a drink of water immediately afterwards. This was repeated every quarter of an hour for two hours, each dose producing a good effect, in that it lessened the pain in the stomach. Later still an emetic of ipecacuan was administered with the immediate result of a vomit, and with great relief from pain. The saccharine oxide was now continued every half hour, and some local frictions to the stomach were prescribed. Six hours later the patient was better, and was allowed to sleep. He slept nearly the whole night, and ultimately made a good recovery.

On the whole Köhler is strongly in favour of the employment of the saccharine oxide of iron: he concludes that it precipitates the arsenic as an insoluble arseniate ($4\text{Fe}_2\text{O}_3 \cdot \text{AsO}_3 + 5\text{HO}$) in the same manner as does the hydrated oxide of iron, and that on chemical grounds it may be fairly substituted for the hydrated oxide. He objects to the employment of solutions of albumen after arsenical poisoning, in conjunction with the treatment by the administration of the oxide of iron antidote, because the albumen delays or prevents the precipitation of the insoluble arseniate. He further objects to the employment of neutral salts as purgatives in connection with the employment of the saccharine oxide of iron as an antidote, on the ground that acidified and concentrated mixtures of gastric juice with neutral alkaline salts prevent the formation of the insoluble arseniate. Hence he prescribes an emetic of ipecacuan early after the use of the antidote, before the arseniate which is formed and which he holds is still poisonous, can be absorbed. He concludes that the saccharine oxide of iron may be preserved for a long time; that dissolved in water it gives a styptic taste but is readily swallowed; and in full doses is preferable to all other preparations of iron as an antidote to arsenious acid. — *Separate Pamphlet*, from *Dr. H. Köhler*, Halle, 1869.

Poisoning by Extract of Belladonna.—Dr. Hibbert Taylor, of Liverpool, reports a case of poisoning by extract of belladonna, an occurrence extremely rare. A young collier, sixteen years of age, swallowed by mistake about a drachm of the extract given to him to make a lotion for his eyes. The poison, dissolved in half a teacupful of warm water, was taken at a quarter before eleven p.m., immediately after supper. He afterwards took a little water, does not seem to have slept, and about ten minutes before twelve he suddenly became violently agitated, throwing his limbs about and groaning and moaning. He was apparently unconscious, but did not speak nor attempt to do so. These symptoms continued for an hour and a half without intermission, and his relative says that he was obliged to lean upon his limbs, to

restrain the violence of his movements. He then became comatose, and so continued until his death, which occurred an hour afterwards, at half-past two o'clock a.m.

The state of the pupil was not ascertained. The brother-in-law says the patient never opened his eyes, but was continually winking or keeping his eyelids nearly closed.

He did not vomit nor attempt to do so; neither did he pass any urine, nor were his bowels moved. He did not speak at all from the commencement of the attack.

He was not seen by any medical man during life, nor was anything attempted for his relief. His friends were under the impression that he was suffering from a fit induced by the medicine which he had taken, as he had told them before taking it that it would make him ill.

The following report of the post-mortem appearances was drawn up by Mr. John Lea Molyneux, of Upholland, who made the examination eighteen hours after death. The upper portion of the body down to the middle, and the upper extremities were greatly discoloured, the vessels of the skin being completely gorged with blood, and decomposition had already commenced. The pupils were greatly dilated, and the rigor mortis was well marked. So far as could be seen, there was no external injury. The lungs were gorged, nearly blackened, in fact. There was a little dark blood in the pleural cavities. The heart was healthy: it was empty, except the right auricle, which contained about six drachms of dark semi-coagulated blood. The liver was in a natural condition. The abdomen was considerably swollen. The stomach seemed very fragile and softened in texture, and was very easily broken. Externally, it displayed several slight discolorations, corresponding to dark patches internally. The contents amounted to about a teacupful, in which Mr. Molyneux could not distinguish anything particular, but he did not make an analysis. All the other abdominal organs appeared to be healthy. On opening the skull, Mr. Molyneux found the scalp and periosteum very much congested. The membranes and the brain itself appeared to be perfectly healthy, and contained very little blood.

There are only two remarks which should be appended to this case. The first has reference to the extreme rarity of the accident. Instances of poisoning from eating the berries of belladonna are common enough, and we are sufficiently well acquainted with the symptoms induced, but Dr. Hibbert Taylor has only been able to find two examples of poisoning from swallowing the extract. These examples are recorded in Taylor's *Medical Jurisprudence*, (p. 359, edit. 1865). The quantity taken is stated to have been a small teaspoonful. In both of these instances the symptoms appear to have been much less violent than in the case above narrated. One patient recovered in two days, and the other is said to have died on the seventh day of some disease.

The other remark regards the length of time which elapsed between the taking of the poison and the manifestation of the characteristic symptoms. The extract was swallowed at a quarter before eleven o'clock, and the symptoms did not appear until nearly midnight, when they seem to have burst out suddenly, and with extreme violence. May

not this delay in the development of the effects of the poison be attributed to the fact, that the patient immediately before taking the belladonna had eaten a hearty supper, and then went calmly to bed—circumstances both apparently unfavorable to the rapid action of the poison?—*British Medical Journal*, November 20th, 1869.

Case of Poisoning by Three Grains of Atropia.—Dr. S. W. Gross records a case in which a lady, forty-three years of age took, owing to an error of a dispenser, three grains of atropia. The symptoms commenced about twenty minutes after taking the dose, at 8.20 a.m., the first symptoms being violent agitation, flushing of the face “as if she had been standing over the kitchen fire,” and in a few minutes a meddlesome and pleasant delirium in which she picked at her clothes, tried to get out of bed, and imagined she was sewing, or nursing her child, or engaged in shopping with her sister. These hallucinations lasted for ten minutes, when she sighed and yawned repeatedly, and “dropped into a comfortable sleep” in which state she remained until the arrival of her brother, Dr. De Young, about ten o’clock, who detected the symptoms of narcotic poisoning, and by going to the druggist’s shop found out that atropia had been given in mistake for assafoetida. The lady lived on for fifteen hours after taking the poison, and fourteen hours afterwards showed signs of returning consciousness. The immediate cause of death was suffocation. The post-mortem appearances were of a negative character.

The treatment pursued in this case included fourteen distinct acts. (1) A purgative injection was administered. (2) A mustard emetic with syrup of ipecacuanha was given. (3). Other emetics, consisting of five ounces of syrup of ipecacuanhæ, two ounces of mustard, and one drachm of the sulphate of zinc were given, but did not produce emesis. (4) Half a grain of acetate of morphia was injected hypodermically. (5) Half a grain of sulphate of morphia was injected hypodermically. (6) The stomach was well washed out. (7) Whiskey and ammonia largely diluted, were injected into the stomach. (8) Faradization of the “phrenic” and “pneumo-gastric” was resorted to, with artificial respiration. (9) A third half grain of morphia was injected hypodermically. (10) Flagellation of the trunk and extremities with bundles of willow switches was resorted to in addition to the other measures employed. (11) Artificial respiration was followed by the Marshall Hall method. (12) Two ounces of whiskey were thrown into the rectum and small lumps of ice were placed in the mouth. (13) Veratria ointment, one drachm to the ounce, was briskly rubbed along the spine, and over the chest and epigastrium. (14) Chapman’s ice-bag was placed along the spine; this was applied about twenty minutes, and with the exception of artificial respiration, was the last remedy employed. The remedies named were resorted to over a period of rather more than thirteen hours, being followed in the order given above. At the close of his report Dr. Gross shows that the urine voided by the patient when injected under the skin of a cat caused dilatation of pupil; the cerebro-spinal fluid applied in a similar manner produced also the same result.—*American Journal of the Medical Sciences*, October, 1869.

[In reviewing this case the mind is struck with the thought that in our urgent and intense desire as curers to cure, we do sometimes out-step the natural bounds of cure. In the case related above the poison atropia ran its regular course, irrespective of all the efforts to destroy its action. Dioscorides or his contemporaries exhibiting atropa mandragora to annul the pain of a surgical operation, would have looked on "the comfortable sleep" of the patient as we look on the sleep produced by chloroform or ether, and would have waited for the natural awaking with as little anxiety. But we, in case of poisoning, are wont to do too much. In this case a single emetic, if it were required at all, would have answered every purpose, after which but three lines of treatment really were open; viz., to have *kept up animal warmth*, to have fed the body with diluents of a nutritious character, such as milk, and to have carefully sustained the respiration, if it became arrested or so feeble that the temperature of the body was reduced, not by forcible movement of body, but by action of small bellows. The application of an ice bag to a person collapsed from a poison which itself kills by reducing animal heat was, we think, unfortunate; and indeed we feel it is doubtful whether even a healthy person could have sustained the whole course of treatment which we have faithfully chronicled from Dr. Gross' candid and ably written report.]

On Essence of Turpentine as an Antidote in cases of Poisoning by Phosphorus.—M. Personne believes that in the present day phosphorus has almost entirely superseded arsenic as a poison in criminal or accidental homicides. Criminal statistics place phosphorus at the head of the list of the substances employed for purpose of poison. From experiments he had performed, M. Personne hoped to find in the essence of turpentine an antidote to phosphorus on the following grounds:

1. It had been long known that the essence of turpentine, like other hydro-carbons, takes from phosphorus its property of luminosity in darkness, of emitting vapour, and it may be said of burning at a low temperature.

2. Dr. Letheby has recently made known that in an English lucifer match factory at Stafford the workmen have been preserved from necrosis of the jawbone, produced by the vapour of phosphorus, by carrying near to their breasts a small open vessel containing essence of turpentine, the fumes from which came in contact with the mouth and nasal orifices.

3. Finally and more recently Dr. Audaut has published some observations on a case of attempted suicide by means of chemical matches. The suicide was rendered abortive by essence of turpentine taken by the unhappy subject in the hope of accelerating and rendering death more certain.

Personne's own experiments were fifteen in number, and were made upon dogs of medium size, and as nearly as possible of the same strength. These animals were treated in five series of three. Number 1 was subjected to the action of phosphorus only; to number 2 the essence of turpentine was administered an hour or two after the phosphorus had been given; and to number 3 the administration of the

essence followed immediately upon the ingestion of the phosphorus. Thus five dogs took phosphorus only, five received the antidote an hour or two after the poison, and five immediately after. All the animals had fasted since the previous evening.

The administration of the phosphorus and of the essence was made by the aid of an œsophageal tube introduced into the stomach by the mouth. Once only it was given in the form of paste for chemical matches; for the others it was dissolved in oil of sweet almonds, and then made into an emulsion with the yolk of egg. These experiments were commenced on the 13th of January and lasted until the 27th of February. The results were that number 1 of every series, namely, those which were subjected to the action of the phosphorus only, died.

Number 2, to which the antidote had been administered an hour or two after the poison had been taken, experienced the same symptoms as the first; some were very ill, but only one succumbed; the remaining four recovered perfectly, and were kept for fourteen or fifteen days after the experiment.

Of number 3, which had received the antidote immediately after the poison, one only died; the others suffered from a slight indisposition, which manifested itself simply by a disinclination for food, but without losing their natural vivacity. These were kept from ten days to a month without exhibiting any change in their health.

It will be seen, then, that all the animals died which did not take the antidote; and that two only died out of the ten remaining, although to five of these the antidote was not administered until one or two hours after the poison.

The absence of success in the two animals which died may be explained by the fact of the extreme cold which prevailed at the time they were submitted to experiment; the temperature was below freezing point.

The largest dose of phosphorus employed in this series was 0·30 grammes, without increasing the dose of the antidote; thus it may be easily understood that the depressing action of the phosphorus upon the animals, added to the intensity of the cold, would have produced fatal results. Of the mode in which essence of turpentine resists the poisonous action of phosphorus, M. Personne says that it does not certainly act like the ordinary antidotes which usually unite with the poison to form insoluble or inactive combinations. The explanation which appears to be the best is, that phosphorus kills, by altering the hæmotosin of the blood, which it deprives of its oxygen rapidly, if its absorption into the blood is rapid, and *vice versâ*. In the first case, death is sudden enough as by a real asphyxia. In the second case, it is slower and fatty degeneration results. The essence of turpentine when absorbed seems to prevent the phosphorus from burning in the blood, in the same way that it prevents its combustion in the air at a low temperature, it takes from it the property of depriving the blood of the oxygen which is indispensable to life; and it can then be eliminated without producing disturbance in the economy.

In one case where the absorption was rapid the blood became

black, and M. Personne was so fortunate as to detect the presence of phosphorus with Metscherlich's apparatus. Rapid death from phosphorus poisoning is due, therefore, according to M. Personne, to a true asphyxia. To test the truth of the hypothesis further, he has conducted some other experiments with pyrogallie acid, which, like phosphorus, has the power of absorbing oxygen. Two dogs were treated with the acid, the one receiving two, the other four grammes. In both symptoms of asphyxia were developed, and the other symptoms tallied with what are observed after administration of phosphorus. The animal which received the dose of four grammes died in fifty hours, the other ten hours later. The post-mortem indications were the same as those which are found in cases where phosphorus has proved fatal.—*Journal de Pharmacologie de Bruxelles*, May; *Annali di Chimica*, August; and *Pharmaceutical Journal*, December, 1869.

On the Cause of Jaundice from Phosphorus.—Dr. Alter in an inaugural dissertation on the above named subject published in Breslau in 1867, contends that the cause of jaundice after the administration of phosphorus in poisonous doses, is the stoppage of the gall duct by a fluid mucous or exudative (catarrhal) secretion. This hypothesis has been declared untenable and false by Dr. Kohts of Königsberg. Alter now returns to the question, reaffirms his previous conclusions, and strengthens them by referring to the publication of Ebstein, who has demonstrated from the human subject the correctness of his views.—*Deutsches Archiv für Klinische Medicin*, September, 1869.

II.—HYGIENE.

Application of Ozone for Purifying Air vitiated by Animal Respiration.—Professor Polli, following up some researches of our own on the inhalation of vitiated air, septic air, the precise nature of which has not been determined by microscopical and chemical researches, proceeds to offer some account of his own investigations into the applicability of ozone as an agent for purifying the air from the vitiating principle emitted through the breath of living bodies. At the theatre La Scala on the occasion of an unusually full attendance he collected the condensable part of the exhaled organic matter, by means of a large glass bell filled with ice, and placed over the circular opening, which, in the roof, corresponds with the large central light. The deposit on this ball was liquid, and had a mouldy smell, was for some few days limpid, but then became very thick, and had a nauseous odour; when mixed with a solution of one part of glucose to four parts of water, and kept at a temperature of from 20° to 24° C., it showed a slow fermentation with the formation on the superficies of green must; during the same period of time and placed under the same conditions a simple glucose solution had undergone no change whatever.

The important point was to determine the possibility of depriving confined air of this vitiating principle, without renewal of air under the process of ventilation and airing at present known. Schonbein's experiments with ozone, which when formed naturally seems destined to

purify the air we breathe, were suggestive so far as regarded the production of ozone, for the purpose of diffusing it in confined atmospheres vitiated by animal respiration. The ozone obtained from electricity was too expensive, and required too much apparatus; and that obtained from phosphorus by semi-immersion, which Barker had already tried, as Schonbein had suggested, had the inconvenience of being too dependent on the surrounding atmosphere and of diffusing into the air a certain amount of phosphoric acid, and of phosphorous vapour.

There remained ozone obtained by the decomposition of alkaline permanganate, by means of a powerful acid. Permanganate of potassa, which is cheap and easily prepared, produced ozone copiously, when acted on by sulphuric acid. The salt is to be well moistened by the acid, (which should be added very gradually), and the mixture should be placed in a well stopped bottle or vessel, and distributed in various parts of the enclosed area. The diffusion of the ozone can be regulated so as to destroy as quickly as it is produced, the miasmatic principle. Dr. Polli thinks the following experiment likely to be an incentive to further research in the application of ozone as a purifying agent. He took two doves of equal size and strength, and enclosed them in separate glass chambers of the same size (10 litres). In one the dove was confined in air only; it lived four hours and a quarter and then died in convulsions. The other dove was placed in a bottle in which there was also one gramme of permanganate of potassa, upon which some drops of sulphuric acid had been poured. The bottle was closed by a glass crossed by a fine tube, so that the ozone could only escape in a very fine jet. An ozone paper prepared with iodide of potassium and starch, according to Schonbein's formula, was suspended inside the chamber, and showed by its colouring that free ozone was accumulating in it; four hours and three quarters afterwards the dove was still alive, and although evidently suffering, had every appearance of being able to sustain life some minutes longer: but Dr. Polli wishing to make an experiment *per contra*, removed the dove and placed it in the open air. In a few moments it breathed freely, and fluttering its wings, was inclined to fly away. It was then placed in a coop with food and water, and left to itself for twenty hours, during which time it ate and drank and seemed perfectly well; it was then placed in a chamber as before, but allowed to breathe the confined air only, and without ozone. In four hours and three quarters it appeared to be dying, it breathed with difficulty, its eyes were closed, and its head bent on one side. It was then removed into the air, but it could not stand and had great difficulty in breathing. It was left stretched out and breathing fitfully until the next day, when, although recovering, it would not eat, and drank very little. It gradually recovered. The experiment shows that in presence of ozone, the dove lived longer than in the confined air, and that although the air was deprived by ozone of the presence of oxygen to the extent of producing asphyxia, it did not cause death as in the case of the dove shut up in air only. By ozone effects can be produced which otherwise could only be attained by a change of atmosphere, and the ozone made from alkaline perman-

ganates seems to be the most suitable for directly destroying in the air miasmatic principles, without otherwise interfering with the respiratory functions. The ozonized air has neither a powerful nor an offensive smell, and it may be easily and economically made. The smell of ozone is scarcely perceptible, and is far less disagreeable than chlorine, bromine, and iodine, while it is more efficacious than either of these; if therefore its application as a purifier of a vitiated air succeeds, it may probably supply all the exigencies of defective ventilation, in crowded atmospheres. In confined places vessels might be placed containing mixtures of permanganate of potassa or soda and acid in proper quantities, and of which the duration of the action was known or sulphuric acid could be dropped upon the permanganate. The presence of the ozone diffused in the surrounding atmosphere might be ascertained or measured by means of ozone cards prepared as before stated, and by experiment in empty and crowded places it would not be difficult to discover the proper proportions.

It must not be forgotten that the quantity of ozone artificially diffused into the surrounding air as guardian of its purity, ought to be carefully determined, because the ozone which is found free or in excess in a given space, that is, not occupied in oxidising or destroying abnormal organic bodies in the air, attacks the other organic bodies with which it comes in contact, does not cease, *i. e.* to exercise its active oxidising function. And as Schonbein in his early experiments found that a certain quantity of ozonized air produced upon himself and the friends who were with him an irritation of the mucous membrane of the lungs (a sort of influenza), the same might occur in a theatre or school, where an excess of ozone would cause irritation in the air-vessels of the persons assembled, and might also cause hoarseness or cough. This would only be, he thinks, of a slight and transient nature, but it would be better to avoid at a first trial whatever ought not to occur at an attempt on a large scale.—*From reports of the Royal Institute of Lombardy. Serie II, Vol. I, Fasc. X.Z.*

Areas for Sewage Irrigation.—Dr. Alfred Carpenter, in a paper on sewage, urges that as it is necessary for the success of sewage irrigation to have sufficient area for its complete use, it becomes an important point to determine how much land is required for a given population, so that all the fertilising elements may be taken out of the water, and the latter made perfectly safe to be returned to the nearest watercourse. This area must vary somewhat according to the density of the population, the character of the rainfall, and the still mooted question as to whether all the latter has to be provided for or not. The experience of some years has now shown that one acre of land will amply provide for the sewage of one hundred persons. In small towns and thinly populated districts the rainfall ought to be kept out of the sewers altogether, on the principle advocated by Mr. Menzies, but if the district is thickly peopled, 100 acres will be quite sufficient for both sewage and rainfall of an area occupied by 10,000 persons.—*Separate Essay, 1869.*

CONTRIBUTIONS TO MEDICAL LITERARY HISTORY.

ADVERSARIA MEDICO-PHILOLOGICA.

BY W. A. GREENHILL, M.D. OXON.

PART VIII.

(Continued from vol. xliv, p. 560.)

γλήνη was first applied to the eye, but in a somewhat indefinite sense, signifying the *pupil*, and also the *image appearing in the pupil*;¹ it is found twice in the Homeric poems, meaning, perhaps, merely the *eyeball* or *central part of the eye*.² Probably from the general idea of a *cavity*, the word was afterwards specially applied to the *cavity of a ball-and-socket joint*, as, for example, the glenoid cavity of the scapula, which receives the head of the humerus.³ It was distinguished from κοτύλη, as being more shallow, but was not constantly used in this restricted sense in Galen's time.⁴ When, however, κοτύλη and γλήνη were thus distinguished from each other, they would correspond respectively to κεφαλή⁵ and κόνδυλος, the more or less prominent articular process, and to ἐνάρθρωσις and ἀρθρωδία,⁶ the deeper or shallower species of ball-and-socket joint.

γληνοειδής, like a γλήνη, *glenoid*, applied to a shallow articular cavity. It is found in the Hippocratic Collection, used in this sense, and opposed to κοτυλοειδής.⁷ It is joined to κοιλότης by Galen,⁸ and applied to the wrist, and also to the vertebræ.⁹ The apparently contradictory expression γληνοειδεῖς ἀποφύσεις, *glenoid processes*, used by Galen,¹⁰ is only an elliptical way of mentioning the *glenoid* [*surfaces, or cavities, of the*] *processes* [*of the spinal vertebræ*].

γλουτός,¹¹ also in the plural γλουτεῖ, *the buttock*,¹² called also

¹ Rufus Ephes., p. 25, l. 2; p. 48, ll. 12, 14, ed. Clinch; Julius Pollux, 'Onom.,' ii, 4, § 70.

² 'Il.,' xiv, 494; 'Od.,' ix, 390. See a long note on this word in Daremberg, 'La Médecine dans Homère,' p. 19.

³ Theophilus, 'De Corp. Hum. Fabr.,' p. 38, l. 8; p. 201, l. 9, ed. Oxon.

⁴ Galen, 'De Oss.,' proœm., t. ii, p. 736, l. 15; quoted by Oribasius, 'Coll. Medic.,' xxv, 2, t. iii, p. 393, l. 6.

⁵ Galen, 'De Motu Musc.,' i, 9, t. iv, p. 410, ll. 8—10.

⁶ Galen, 'De Oss.,' loco cit., ll. 3—6. 13—15.

⁷ 'De Artic.,' § 79, t. iv, p. 316, l. 11, ed. Littré.

⁸ 'De Usu Part.,' ii, 11, t. iii, p. 132, l. ult.; copied by Theophilus, 'De Corp. Hum. Fabr.,' i, 12, p. 29, l. 13, ed. Oxon.

⁹ 'De Oss.,' c. 8, t. ii, p. 757, l. 10; copied by Oribasius, 'Coll. Medic.,' xxv, 9, t. iii, p. 406, l. 9.

¹⁰ 'De Oss.,' c. 9, t. ii, p. 760, ll. 10, 12.

¹¹ Hippocr., 'De Fract.,' t. iii, p. 88, l. 5, ed. Kühn; Aristotle, 'Hist. Anim.,' i, 13, p. 16, l. 2, ed. Bekker; Aretæus, 'Sign. Chron.,' ii, 12, p. 170, l. 6, ed. Kühn; Galen, 'De Anat. Admin.,' ii, 5, t. ii, p. 302, l. 13.

¹² Rufus Ephes., p. 32, l. 20; p. 52, l. 10, ed. Clinch; Pseudo-Galen, 'Introd.,' c. 10, t. xiv, p. 707, l. 13.

πυγαί.¹ It was afterwards applied to the process of the femur called the *great trochanter*.² In Meletius³ a derivation of the word is given which seems to be corrupt, but which at the moment I am unable to correct.⁴ In the middle ages the form γλουτόν was in use,⁵ and in the present text of Galen γλουτά is found in one passage, where, however, we should probably read γλούτια.⁶ The diminutive form γλούτιον (probably found only in the plural, γλούτια) was applied in Galen's time to two of the *corpora quadrigemina*, which still bear the name of *nates*.⁷

γλῶσσα, or γλῶττα, *the tongue*; sometimes meaning *the mouth-piece of a pipe*, αὐλός;⁸ and thence applied to the larynx, and used as synonymous with γλωττίς.⁹

γλωσσοκάτοχος, or perhaps rather γλωσσοκάτοχον, *a tongue spatula*. The invention of the instrument is attributed sometimes to Paulus Ægineta, but perhaps without sufficient reason, as he gives no description of it,¹⁰ and speaks of it as "*the tongue spatula*," as if familiar to his readers. In the Arabic translation by Albucasis, the Greek name is neither retained nor translated, but there is a short description of the instrument, with two figures.¹¹

γλωσσόκομον (sometimes also called γλωσσοκομέον),¹² a curious word, meaning originally *a case to hold* (κομέω) *the mouth-pieces* (γλῶσσα) *of wind instruments*; then applied to a *box* or *case* in general;¹³ and in surgery specially signifying an instrument invented by Nymphodorus, probably in the third century B.C., and much used in cases of fracture or dislocation of the lower extremity.¹⁴ It was furnished

¹ Galen, 'De Usu Part.,' xv, 8, t. iv, p. 252, l. 6.

² Galen, 'De Oss.,' c. 21, t. ii, p. 773, l. ult.; copied by Theophilus, 'De Corp. Hum. Fabr.,' i, 23, p. 53, l. 5, ed. Oxon., and by Oribasius, 'Coll. Medic.,' xxv, 20, t. iii, p. 417, l. 5, ed. Daremb.

³ 'De Hom. Fabr.,' c. 29, p. 126, l. 29, ed. Cramer.

⁴ γλουτοί, οἷον γλυοί τινες ὄντες.

⁵ Sanguinatus (or *Hypatus*), 'De Corp. Part.,' p. 152, ed. Bernard; or p. 129, in Darembert's 'Notices et Extraits,' &c., where there is a good note by the editor.

⁶ 'De Anat. Admin.,' ix, 5, t. ii, p. 729, l. 7.

⁷ 'De Usu Part.,' viii, 14, t. iii, p. 678, l. 1, &c.; copied by Oribasius, 'Coll. Medic.,' xxiv, 1, t. iii, p. 280, l. 6, &c. 'De Anat. Admin.,' ix, 5, t. ii, p. 729, ll. 7, 10; p. 730, l. 13, where for γλουτά, γλουτῶν, γλουτοῖς, we should probably read γλούτια, γλουτίων, γλουτίοις.

⁸ Galen, 'De Usu Part.,' vii, 13, t. iii, p. 561, l. 2; copied by Oribasius, 'Coll. Medic.,' xxiv, 9, t. iii, p. 315, l. 13, ed. Daremb.

⁹ Galen, *ibid.*, p. 562, l. 13; Oribasius, *ibid.*, p. 316, l. 8.

¹⁰ Lib. vi, cap. 30, p. 156, l. 5, ed. Briau.

¹¹ 'De Chir.,' ii, 36, p. 200, ed. Channing; or p. 106 in Le Clerc's French translation.

¹² Galen, 'De Usu Part.,' vii, 14, t. iii, p. 573, ll. 8, 11. The form γλωσσοκόμιον (Galen, 'Comment. in Hippocr. *De Fract.*, ii, 64, t. xviii B, p. 502, l. 6) is inadmissible.

¹³ It is the word used in the LXX (2 Chron. xxiv, 8, 10, 11) for the 'chest,' or alms-box, made by command of King Joash; and also the word mistranslated 'bag,' which was kept by Judas Iscariot (St. John xii, 6; xiii, 29).

¹⁴ Galen, 'De Meth. Med.,' vi, 5, t. x, pp. 442, 443; Oribasius, 'Coll. Med.,' xlix, cc. 6, 21, t. iv, pp. 355, 399.

with pulleys, from which circumstance Galen referred to it in illustration of the course and functions of the inferior laryngeal or recurrent nerves.¹ An illustrative plate of the instrument may be found in Daremberg's 'Œuvres de Galien,' t. i, p. 501, and in his edition of Oribasius, t. iv, pp. 692, 694.

γλωττίς, in medical writers, signified *the principal organ of the voice*;² but was not always used in a precise and definite sense, nor did each writer give it exactly the same signification. The origin of the term, as used in anatomical writings, has been disputed, but can be determined with tolerable certainty. The full name is γλωσσα or γλωττίς λάρυγγος, and would almost seem to have been given by Galen himself;³ the part was so called because, in the production of the human voice, it answered the same purpose as the mouth-piece or tongue of the ancient pipe (αὐλός).⁴ Galen certainly did not mean by this term merely the *rima glottidis*, or merely the *ventricles of the larynx*, as he speaks of it, not as a cavity, but as a *body* (σῶμα);⁵ he probably intended all the walls of the ventricles of the larynx, together with the cavity itself. In later writers⁶ the word was sometimes confounded with the *epiglottis*, insomuch that Vesalius thought it necessary⁷ to explain more than once that the two words have different significations, attributing the cause of the error partly to Celsus,⁸ who applied the term *exigua lingua* (or *lingula*) to the *epiglottis*. M. Béclard says that γλωττίς also signified the *uvula* (*luette*), but he does not give any reference.⁹

γνάθος is explained by Galen to signify *cheek*, viz. the part beneath the μῆλον, or *cheek-bone*,¹⁰ and in this sense he frequently (perhaps generally) uses the word himself.¹¹ Rufus Ephesius gives the same explanation, and says that the word was synonymous with *σιαγόνες* and *παρειαί*.¹² Hippocrates¹³ uses it in the sense of *jaw*, and speaks of ἡ κάτω γνάθος and ἡ ἄνω γνάθος, *the upper and the lower jaw*.

γομφίος, more commonly found in the plural γομφίοι,¹⁴ *the molar*

¹ 'De Usu Part.,' vii, 14, t. iii, p. 573.

² Galen, *ibid.*, vii, 13, t. iii, p. 560, l. ult.

³ *Id.*, *ibid.*, p. 562, l. 13. He does not say "which is called the mouth-piece and tongue of the larynx," but "which I call," &c.

⁴ *Id.*, *ibid.*, p. 553, l. 14; p. 561, ll. 2, 7, 14. To say, therefore, "Evidemment il n'y a rien dans le larynx qui ressemble à une langue ou à une petite langue" ('Dict. Encycl. des Sciences Méd.,' 1868, under "Larynx," p. 549), is to misapprehend the origin of the term.

⁵ *Ibid.*, p. 553, l. 14; p. 560, l. 12. The greater part of these chapters of Galen are copied by Oribasius, 'Coll. Med.,' xxiv, 9, t. iii, p. 315, &c.

⁶ In Theophilus, 'De Corp. Hum. Fabr.,' iii, 15, p. 112, ed. Oxon., the former part of the description (which is probably original) clearly relates to the *epiglottis*. the latter part (which is copied from Galen) relates to the *glottis*.

⁷ 'De Hum. Corp. Fabr.,' lib. i, cap. 38; lib. ii, capp. 21, 22.

⁸ 'De Medic.,' iv, 1, p. 120, l. 17, ed. Daremb.

⁹ 'Dict. Encycl. des Sciences Méd.,' under "Larynx," p. 549, ed. 1868.

¹⁰ 'Comment. in Hippocr. De Artic.,' ii, 1, tom. xviii A, p. 423, l. 3.

¹¹ 'De Anat. Admin.,' iv, 2, t. ii, p. 421, ll. 9, 10; copied by Oribasius, 'Coll. Med.,' xxv, 25, t. iii, p. 422, ll. 7, 8; and elsewhere.

¹² 'De Part. Corp. Hum.,' p. 28, l. 20; p. 48, l. 19, ed. Clinch.

¹³ 'De Artic.,' § 30, t. iv, p. 140, ll. 6, 7, ed. Littré.

¹⁴ Aristotle, 'De Part. Anim.,' iii, 1; iv, 11, p. 51, l. 23; p. 117, l. 17, ed. Bekker.

teeth, so called because they are fixed in their sockets like a nail (*γόμφος*) in wood. They were also called *μύλαι*,¹ and *μυλῖται*,² and *ἀλήθοντες*,³ *the grinders*, whence the Latin *molares*,⁴ and the modern *molar*; they are called by Celsus⁵ *maxillares*. The last of the molar teeth on each side were called *κραντῆρες*,⁶ or *σωφρονιστῆρες*⁷ (answering to the modern *dentes sapientiæ*), and by Pliny *genuini*.⁸ The other teeth were called *τομεῖς* or *τομικοί*, *incisores*, and *κυνόδοντες*, *canini*. (See these words.)

From *γομφίος* comes *γομφιάζω*,⁹ *to have the toothache*, whence the words *γομφίασις*¹⁰ and *γομφιασμός*,¹¹ *toothache*. With respect to the passage in Dioscorides, it should be noticed that it was proposed by Caspar Hoffmann to read *ἀγομφίασις* or *ἀγόμφωσις*, in the sense of *looseness of the teeth*.¹² The emendation is unnecessary, and it is also entirely unsupported by external testimony. Kühn appears to approve of it,¹³ but Sprengel has not adopted it in his edition; and it may be added that it was not found in the copy used by Ebn Baithar in the thirteenth century.¹⁴ The two words *agomphiasis* and *agomphosis* are found in some modern medical lexicons, but it is doubtful whether they rest on any better authority than this conjecture.

γόμφος is found in later writers¹⁵ in the sense of *the molar teeth*. Bernard thinks the word corrupt, Ermerins accepts it as genuine.

γόμφωσις, one of the three species of *συνάρθρωσις*¹⁶ (the other two being *σύμφυσις* or *ἀρμονία* and *ῥαφή*), in which the articulation takes place (as in the case of the teeth) by one bone being fixed in another, like a nail or peg (*γόμφος*) in its hole. The separation of bones thus united was called *διακίνημα*.¹⁷

γονάγρα. See *γοννάγρα*.

γονή, *the seminal fluid of animals*, in which sense it is commonly used as synonymous with *γόνος*, *σπέρμα*, and *θορή*,¹⁸ though some

¹ Galen, 'De Oss.,' c. 5, t. ii, p. 753, l. ult.

² Meletius, 'De Corp. Hum. Fabr.,' c. 10, p. 81, l. 27, ed. Cramer; Pseudo-Galen, 'Introd.,' c. 12, t. xiv, p. 722, l. 4.

³ Hypatus, 'De Corp. Part.,' p. 148, ed. Bernard.

⁴ Juvenal, 'Sat.' v, 160; xiii, 212.

⁵ Lib. viii, c. 1, p. 455, l. 2, ed. Targa.

⁶ Aristotle, 'Hist. Anim.,' ii, 4, p. 34, l. 4, ed. Bekker.

⁷ Theophilus, 'De Corp. Hum. Fabr.,' iv, 29, p. 178, l. 8, and note, ed. Oxon.

⁸ 'Hist. Nat.,' xi, 63, ed. Tauchn.

⁹ LXX in Ezek. xviii, 2.

¹⁰ Dioscor., 'Mat. Med.,' ii, 63, t. i, p. 191, l. 13, ed. Sprengel.

¹¹ LXX in Amos iv, 6.

¹² See Castelli, 'Lex. Med.,' in *Gomphiasis*.

¹³ See his 'Censura Medic. Lexicor. Recent.,' § vi, reprinted in his 'Opusc. Acad. Med. et Philol.,' vol. ii, p. 352.

¹⁴ In the extract from Dioscorides (vol. i, p. 105) the word is rendered by Sontheimer, in his translation of Ebn Baithar, *zahnnschmerzen*, or toothache.

¹⁵ Hypatus, 'De Corp. Part.,' p. 148, ed. Bernard; Pseudo-Hippocr., 'Epist. ad Ptolem.,' p. 287, l. ult., in Ermerins, 'Anecd. Med. Græca.'

¹⁶ Galen, 'De Oss.,' procem., t. ii, p. 737, l. 6; p. 738, l. 1; Pseudo-Galen, 'Introd.,' c. 12, t. xiv, p. 720, l. 7; p. 722, l. 1.

¹⁷ Galen, 'Defin. Med.,' c. 474, t. xix, p. 461, l. 1.

¹⁸ Galen, 'De Locis Aff.,' vi, 6, t. viii, p. 439, l. 13; Rufus Ephes., p. 44, l. 10, ed. Clinch.

writers¹ draw a somewhat obscure and subtil distinction between *γονή* and *σπέρμα*. Both words are used by Hippocrates, and apparently indiscriminately, and both are rendered by the same Arabic word² *مذى* *mindá*. Probably, however, *γονή* was the more usual word in the oldest times, and *σπέρμα* in the later, as in the Hippocratic Collection there is a treatise entitled *Περὶ Γονῆς*, and among Galen's works one *Περὶ Σπέρματος*.³ The phrase *ἀπόκρισις γονῆς*, *the secretion (or emission) of semen*, is found in the Hippocratic Collection,⁴ and in Aretæus⁵ (who imitated the language of Hippocrates), and *ἀπόκρισις σπέρματος* in Galen.⁶ In connexion with this signification the word is applied in one passage in the Hippocratic Collection to an ovoid body expelled from the vagina six days after sexual intercourse, which the writer supposed to be an embryo in its earliest stage of development, but which, as the ovum would not have descended into the uterus in so short a time, was no doubt only the decidua uteri.⁷ In the Hippocratic Collection the word sometimes signifies *the uterus* or *the generative*⁸ *organs* in general. Sometimes the plural form, *γοναί*, is used in the same sense.⁹ The expression *ἐκ γονῆς*¹⁰ may either mean *from the womb* (as in the similar phrase, *ἐκ κοιλίας*¹¹ *μητρός*) or *from birth*.

γόνιμος signifies, (1), *fecund, prolific*, as *γόνιμον σπέρμα*, *fruitful semen*, opposed to *ἀγονον σπέρμα*, *unfruitful semen*; ¹² *γονίμη ἡλικίη*,¹³ *the age of fecundity*;—also (2), generally, *generative, relating to generation* (synonymous with *γεννητικός*), as *γόνιμα μέρεα*,¹⁴ *the parts of generation*; *γόνιμοι φλέβες*,¹⁵ *the genital canals* (probably all those composing the spermatic cord);—also (3), *viable, able to live* (synonymous with *βιώσιμος*), as *γόνιμον ἔμβρυον*,¹⁶ *a viable fœtus*; *γόνιμον παιδίον*,¹⁷ *a viable infant*;—also (4), *odd, uneven* (synonymous with

¹ Aristotle, 'De Gener. Anim.,' i, 18, § 38, p. 22, l. 14, ed. Bekker; Joannes Alexandr., 'Comment. in Hippocr. De Nat. Pueri,' in Dietz, 'Scholia in Hippocr. et Gal.,' t. ii, p. 206, l. 25.

² Honain's translation of the 'Aphorisms,' v, 61 (62); vi, 2, p. 50, l. 11; p. 53, l. 2.

³ See the chapter *περὶ σπέρματος* in Oribasius, 'Coll. Med.,' xxii, 2, and the passages from Galen's works from which it is taken (t. iii, p. xiii, ed. Daremb.).

⁴ 'De Genit.,' § 2, t. vii, p. 472, l. 18, ed. Littré.

⁵ 'De Caus. Acut.,' i, 5, p. 5, l. 5, ed. Kühn.

⁶ 'De Usu Part.,' xv, 3, t. iv, p. 222, l. 8.

⁷ See Robin's Note in Littré's 'Argument' to 'De Nat. Pueri,' t. vii, p. 463, &c., and pp. 488, 490.

⁸ 'Mochl.,' § 1, t. iv, p. 342, l. 1; 'Epid. II,' i, 6, t. 5, p. 76, l. 16; 'De Usu Liq.,' § 2, t. vi, p. 126, l. 2, ed. Littré. See Rufus Ephes., p. 40, l. 22, ed. Clinch.

⁹ 'De Artic.,' § 45, t. iv, p. 194, l. 5, ed. Littré; 'De Superf.,' § 15, t. viii, p. 484, l. 9. The former passage Galen applies also to the *spermatic vessels* (*τὰ σπερματικά*) in males; but this is doubtful. See Eustachi's note on Erotianus, p. 112, ed. Franz.

¹⁰ 'Epid. IV,' § 31, t. v, p. 174, l. 16, ed. Littré.

¹¹ 'Acts' iii, 2; xiv, 8.

¹² Rufus Ephes., p. 63, ll. 12, 14, ed. Clinch.

¹³ Pseudo-Hippocr., 'De Diæta,' i, 25, t. vi, p. 498, l. 6, ed. Littré.

¹⁴ Aretæus, 'Caus. Chron.,' ii, 5, p. 143, l. ult., ed. Kühn.

¹⁵ Rufus Ephes., p. 39, l. 22; p. 63, l. 13, ed. Clinch.

¹⁶ Aristotle, 'Hist. Anim.,' vii, 4, § 1, p. 202, l. 21, ed. Bekker.

¹⁷ Hippocr., 'De Superf.,' § 4, t. viii, p. 478, l. 4, ed. Littré.

περιττός), because (as Erotianus¹ explains it) the uneven days are generally critical and productive of some good to the patient; thus γονίμη ἡμέρα, *an uneven day*.²

γονοειδής, *resembling the seminal fluid*, is found in the Hippocratic Collection,³ and also in Aretæus applied to the urine⁴ in men, and to a whitish uterine discharge in women.⁵

γονόρροια, a word probably not found in any extant author older than Aretæus⁶ and Soranus.⁷ Celsus,⁸ when he speaks of "nimia profusio seminis," would probably have used the word γονόρροια if it had been in use in his time. It signifies in the ancient writers what its etymology would lead us to expect, viz., *an emission of semen*, or (in modern technical language) *spermatorrhœa*,⁹ a word invented in quite recent times, because *gonorrhœa* had so entirely lost its original meaning. It was applied, however, in a wide sense to women as well as men;¹⁰ and as the word *semen* was not always used in the strict meaning of modern pathologists, *gonorrhœa* would often signify what would now be called *blennorrhœa*. The references to the principal passages relating to *gonorrhœa* are given by Adams in the Commentary to his Paulus Ægineta (iii, 54, 55, vol. i, pp. 592, 594).

γόνος is used in the Hippocratic Collection as synonymous with γονή,¹¹ to signify *the seminal fluid*, not only of the male, but also (as was supposed) of the female.¹² Galen draws a distinction between γόνος and σπέρμα, which is not often observed; he says that the seminal fluid was called σπέρμα while still in the spermatc vessels, and γόνος after it was emitted.¹³ In one passage of the Hippocratic Collection γόνος appears to mean (if the reading be correct) either the *glans penis*, or the *penis* itself, or the *male genital organs*;¹⁴ but Ermerins considers the passage to be corrupt, and proposes to read τόπος for γόνος.¹⁵

γόνυ, *genu* in Latin, *the knee*, or *region of the knee*, a word found in the Homeric Poems.¹⁶ In one passage of the Hippocratic Collection it was supposed to be used in the sense of *offspring*;

¹ 'Voc. Hippocr.,' p. 110, ed. Franz, and note.

² Hippocr., 'Epid. II,' v. 12, t. v, p. 130, l. 15, ed. Littré.

³ See Erotianus, 'Voc. Hippocr.,' p. 110, ed. Franz, and note; Foës, 'Æcon. Hippocr.'

⁴ 'Caus. Acut.,' ii, 12, p. 47, l. 20, ed. Adams.

⁵ 'Caus. Chron.,' ii, 11, p. 116, l. 3.

⁶ 'Caus. Chron.,' ii, 5; 'Cur. Chron.,' ii, 5, pp. 103, 232, ed. Adams.

⁷ In Cœlius Aurel., 'Morb. Acut.,' iii, 18, p. 249, l. 19; 'Morb. Chron.,' ii, 1, p. 347, l. 19, ed. Amman.

⁸ 'De Medic.,' iv, 28 (21), p. 155, ed. Daremb.

⁹ Cœlius Aurelianus (*locis cit.*) renders the word by 'seminis lapsus.'

¹⁰ Aretæus speaks of γονόρροια γοναικία, 'Caus. Chron.,' ii, 11, p. 116, l. 4, ed. Adams.

¹¹ 'De Morb. Mul.,' i, 24, t. viii, p. 64, ll. 3, 5, ed. Littré, and elsewhere.

¹² 'De Genit.,' § 7, t. vii, p. 478, l. antep., and elsewhere.

¹³ 'Defin., Med.,' c. 441, t. xix, p. 450, l. ult.

¹⁴ 'De Ligu. Usu.,' § 2, t. vi, p. 126, l. 2, ed. Littré.

¹⁵ Hippocr., 'Opera,' vol. ii, p. 114.

¹⁶ See Daremberg, 'La Médecine dans Homère,' p. 21.

but Littré and Ermerins read γόνον, which is doubtless the true reading.¹

γοννάγρα, mentioned by Cœlius Aurelianus (and probably by him only) as a species of ἀρθρίτις, and signifying a *gouty* (or *rheumatic*) *affection of the knee*, as ποδάγρα had reference to the *feet*, and χειράγρα to the *hands*. The word is written γονάγρα in Amman's edition,² and this form has been inserted in medical dictionaries; but there is probably no authority for it, and the more correct form is certainly γονατάγρα, or γονιάγρα,³ or γοννάγρα, which last may be preferred from its similarity to the kindred word γονναλγής.

γονναλγής, *suffering pain in the knee*.⁴

γραῖα, a *wrinkled old woman*; also the *scum on boiled milk*; hence applied to the *folds of skin round the umbilicus*.⁵ (Compare *Anus*.)

γραφοειδής, or γραφειδής, *like to a stilus or pencil*. The word is written γραφοειδής in the printed editions of Galen,⁶ and this is the only form that appears in Liddell and Scott's Lexicon. But γραφοειδής (or perhaps γραφειοειδής) is more correct, because the word signifies similitude γραφείων (or γραφίων) πέρασι,⁷ 'to the point of a stilus, or pencil;' and this is the form of the word found in Oribasius.⁸

γραφοειδής ἀπόφυσις is the *styloid process* of the temporal bone, or of the ulna;⁹ γραφοειδής ἔκφυσις¹⁰ is the *styloid process* of the temporal bone; γραφοειδὲς ὀστοῦν¹¹ is the *temporal bone*, so called on account of the styloid process. (See the nearly synonymous words βελονοειδής, στυλοειδής.)

γρύπωσις, *incurvation* in general, but especially of the *nails*, rendered by Cœlius Aurelianus¹² *unguium uncatio*. The verb γρυποῦσθαι is found both in the Hippocratic Collection¹³ and in Galen¹⁴ in the same sense as applied to the nails, and as indicating a frequent symptom in consumptive patients.

¹ 'Epid.,' vii, 6, t. v, p. 378, l. 3, ed. Littré; vol. i, p. 648, l. 16, ed. Ermer.

² 'Morb. Chron.,' v, 2, p. 557.

³ Compare the form δοριάλωτος.

⁴ Hippocr., 'Epid.,' ii, 4, § 3; vi, 4, § 11, t. v, p. 126, l. 6; p. 310, l. 5, ed. Littré.

⁵ Rufus Ephes., p. 31, l. 7; p. 51, l. 16, ed. Clinch; Julius Pollux, 'Onom.,' ii, 4, § 170.

⁶ 'De Oss.,' c. 1, t. ii, p. 745, l. 11, and elsewhere.

⁷ Galen, 'De Usu Part.,' vii, 19, t. iii, p. 592, l. 2. In Oribasius ('Coll. Med.,' xxv, 8, § 2, p. 403, l. 9) and in Theoph. Nonnus (c. 259, ed. Bernard) the word is written γραφίον (compare the Latin *graphium*), which form is not given by Liddell and Scott.

⁸ *Loco cit.*, p. 398, l. 12, ed. Daremb., and elsewhere.

⁹ See the word ἀπόφυσις in Part IV.

¹⁰ Oribas., *loco cit.*, p. 404, l. 4.

¹¹ Galen, 'De Oss.,' c. 8, t. xviii, B, p. 958, l. 5.

¹² 'Morb. Acut.,' ii, 32, p. 149; 'Morb. Chron.,' ii, 14, p. 421, ed. Amman.

¹³ 'Prognost.,' § 17, t. ii, p. 154, l. 4; 'Præn. Coac.,' § 396, t. v, p. 672, l. 20, ed. Littré.

¹⁴ 'De Locis Aff.,' i, 5, t. viii, p. 47, l. 10; 'Comment. in Hippocr. *De Hum.*,' i, 24, t. xvi, p. 205, l. 9; 'Comment. in Hippocr. *Progn.*,' ii, 60, t. xviii, B, p. 204, l. 2.

γυναικεῖος, *female, relating to women.*

τὰ γυναικεῖα, *the female organs of generation*; ¹ also *the menses*; ² also *the lochia*; ³ also *female diseases*.⁴

γυναικεῖον αἰδοῖον, *the female genital organs*,⁵ especially *the vagina*.⁶

γυναικεῖα γονόρροια, *female spermatorrhœa*, a name applied by Aretæus⁷ to a whitish uterine discharge.

γυναικεῖος κόλπος, *the vagina*.⁸

γυναικεῖα παθῆ, *female diseases*.⁹

γυναικεῖος ῥοῦς, *leucorrhœa*, as distinguished from the menses;¹⁰ but used also in a much wider sense to signify any morbid uterine discharge.¹¹

γυναικεῖοι τόποι, *the female genital organs*,¹² called also γυναικεῖο χώροι.¹³

γυναικικός, *womanish*, in the sense of *weakly*; ¹⁴ but also simply as *resembling a woman*, applied to the male mamma.¹⁵

γυναικόμαστος, used probably as an adjective by Paulus Ægineta,¹⁶ *having breasts like women*, relating to men only; the neuter, γυναικόμαστον (or, in the Doric form of the word, γυναικόμασθον), signifies, according to Galen,¹⁷ *an unnatural development of the mamma*, an affection not confined to men. Perhaps the word is not used by any other Greek author.¹⁸

¹ Aretæus, 'Caus. Acut.,' ii, 11, p. 61, l. 12, ed. Kühn.

² Aristotle, 'Hist. Anim.,' vii, 1, 2, p. 198, l. 23; p. 199, l. 15, ed. Bekker.

³ Galen, 'Comment. in Hippocr. Aph.,' v, 28, t. xvii, B, p. 817, l. 14.

⁴ Aretæus, 'Cur. Acut.,' i, 3, p. 209, l. 7, ed. Kühn.

⁵ Rufus Ephes., p. 41, l. 3, ed. Clinch.

⁶ Galen, 'De Usu Part.,' xv, 3, t. iv, p. 223, l. 2, copied by Theophilus, 'De Corp. Hum. Fabr.,' v, 24, p. 220, l. 9, ed. Oxon.

⁷ 'Caus. Chron.,' ii, 11, p. 164, l. ult., ed. Kühn.

⁸ Soranus, 'De Arte Obstetr.,' c. 5, p. 12, l. 18; p. 14, l. 8, ed. Dietz; Rufus Ephes., p. 41, l. 2, ed. Clinch.

⁹ Soranus, Moschion.

¹⁰ Rufus Ephes., p. 44, l. 9, ed. Clinch.

¹¹ Galen, 'Defin. Med.,' c. 301, t. xix, p. 429; and especially Soranus, 'De Arte Obst.,' c. 107, p. 240, ed. Dietz.

¹² Aretæus, 'Caus. Acut.,' ii, 11, p. 62, l. penult., ed. Kühn.

¹³ Id., 'Cur. Acut.,' ii, 10, p. 287, l. 3.

¹⁴ Aristotle, 'Gener. Anim.,' iv, 2, p. 116, l. 21, ed. Bekker.

¹⁵ Id., 'Hist. Anim.,' vii, 1, p. 198, l. 26.

¹⁶ Lib. vi, cap. 46, p. 212, ed. Briau.

¹⁷ 'Defin. Med.,' cap. 403, t. xix, p. 444, l. 9.

¹⁸ See Kühn, 'Censura Medicor Lexicor Recent.,' viii, in his 'Opusc. Academ.,' vol. ii, p. 367; Kraus, 'Medic. Lex.'

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NOTICE TO READERS.

THE Editor is particularly desirous of having all Reports of Hospitals, Asylums, Sanitary Boards, Scientific Societies, &c., forwarded to him, as also Inaugural Lectures, Dissertations, or Theses, Medical and Scientific Addresses, &c.

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